PROCEEDINGS

OF THE

American Philosophical Society

Held at Philadelphia for Promoting Useful Knowledge

VOLUME 97, NUMBER 2 APRIL 30, 1953

CONTENTS

New Trends in Human Evolution. FREDERICK OSBORN 143

An Unsettled Problem in Interstate Divorce Law. HERBERT F. GOODRICH 147

The Place of Diplomatic History in the Study of International Relations during the Twentieth Century. SIR LLEWELLYN WOODWARD 151

The American Occupation and German Education. GEORGE N. SHUSTER 159

The Measurement of Criminality in Geographic Areas. THORSTEN SELLIN 163

On the Nature of Scientific Research. DAVID MEREDITH SEARES WATSON 168

The Friendship of the "Reform" Cardinals in Italy with Damião de Goes. ELISABETH FEIST HIRSCH 173

Studies in the Problem of Dating the Dead Sea Scrolls. JOHN C. TREVER 184

The Possible Source and Antiquity of the Slate Churingas of Western Australia. DANIEL SUTHERLAND DAVIDSON 194

The Cultures of Early Anatolia. ALBRECHT GOETZE 214

An Experiment in Measurement. ALEXANDRE KOYRÉ 222

THE AMERICAN PHILOSOPHICAL SOCIETY

INDEPENDENCE SQUARE

PHILADELPHIA 6

1953

The *Proceedings* of the American Philosophical Society is published as an annual volume and ordinarily is issued in six numbers. An author desiring to submit a manuscript for publication should sent it to the Editor, Luther P. Eisenhart, American Philosophical Society, 104 South Fifth Street, Philadelphia 6, Pa.

The annual subscription price is five dollars; combined with a subscription to the *Transactions* the price is ten dollars. Individual copies of the *Proceedings* are offered for sale. This issue is priced at one dollar.

LANCASTER PRESS, INC., LANCASTER, PA.

NEW TRENDS IN HUMAN EVOLUTION

FREDERICK OSBORN

(*Read November 13, 1952*)

A FUNDAMENTAL change is taking place in the biology of peoples of Western European descent. Some of the implications of this change may have a more far reaching effect than anything that has previously happened to the race in the period of recorded history. Yet it is almost unnoticed by the public, and there is a minimum of scientific research directed towards finding out its significance.

The change has to do with the recent reduction in early deaths and with new trends in the distribution of births. Differential deaths and births are generally accepted as the major factors in evolution. They are also capable of a profound effect on our cultural inheritance, both within European populations, and with respect to the balance between different world cultures.

THE REVOLUTION IN BIRTHS AND DEATHS: ITS EFFECT ON WORLD CULTURES

There has been a spectacular decline in deaths among Western European peoples during the past hundred years. Today 90 per cent of all children born alive live to the middle of their reproductive period; at the founding of the Republic, only about 36 per cent survived to age thirty-five.[1]

A corresponding decline in births has been going on since the beginning of the industrial revolution and the urbanization of Western European populations. It has followed somewhat behind the decline in deaths, but finally caught up to it about the time of the depression. The great increase in births in the immediate post-war years is probably only a temporary reversal of the tendency to decline. For the years 1929, 1930, and 1931, which reflected as much prosperity as depression, only 17.7 per cent of American married women had five children or more, 7.7 per cent had four, 31.5 per cent had two or three, and 43.1 per cent had one or none.[2] The women of childbearing age were having barely enough children to replace their own number (net reproductivity for 1930, 1.079).[3] In contrast, at the end of the eighteenth century, there was an average of 7.8 children per married woman, or 8.3 per mother,[4] about sufficient to double the population in each generation.

Among the peoples of Western European descent, it seems probable that when the post-war upsurge in births has spent its force, births and deaths will be about in balance, at a relatively low level.

While these changes have been taking place among Western Europeans, the far more numerous peoples in the non-European world have been increasing at an unprecedented rate, as a result of the sudden fall in deaths under the influence of moderate public health measures. At some point their numbers too will necessarily be stabilized, either by a return to high death rates, or by a reduction in birth rates comparable to that which has taken place among the people of European descent. But before stabilization takes place, non-Europeans will inevitably comprise an even more preponderant majority of the world's people than they do at present.

It seems very unlikely that there will be enough assimilation of European culture patterns to make up for such a change in the proportion of different races. Since we are all dedicated to the survival of our particular type of civilization, we may well begin asking ourselves the question whether the next fifty years will see an improvement in the ability and character of our people sufficient to offset their relative numerical decline. To an unknown extent this will depend on evolutionary forces.

THE CHANGED BALANCE OF FORCES DETERMINING HUMAN EVOLUTION

We assume that in the long history of man, the distribution of hereditary factors for mental traits was arrived at largely through selection by deaths,

[1] Estimated from Wigglesworth table published in *Amer. Acad. Arts and Sci. Memoirs* **2** (1): 133, 1793.

[2] Whelpton, P. K., and E. Jackson, Prolificacy distribution of white wives according to fertility tables for the registration area, *Human Biology* **12** (1): 54, Feb. 1940.

[3] Lotka, Alfred J., Modern trends in the birth rate, *Annals Amer. Acad.* **188**: 7, 1936.

[4] Lotka, Alfred J., *Jour. Amer. Statist. Assn.*, 169, June 1927.

that is, through the differential survival of those individuals whose intelligence and personality most fitted them to survive. To a great extent this process is still going on among non-Europeans. But among Western European peoples this process is no longer operating. Most deaths, so far as they have a selective effect, occur at so young an age that, though they undoubtedly select for physical characters, there can be no selection for intelligence or personality. Whatever changes may be taking place today in the distribution of factors for intelligence and personality are not due to selective deaths but to selection by differential births, or to factors coming under the head of mate selection or genetic drift, or to chance variations among groups of isolates. All these things are being studied, but the aggregate of time or money being spent on them is probably not one-tenth of one per cent of the time which was spent to determine in advance the results of last year's election.

PRESENT KNOWLEDGE AND NEED FOR RESEARCH

We know nothing of the present effect of birth selection on the distribution of genes from one generation to another. We have, however, some knowledge of the factors affecting the distribution of births, which can be briefly summarized:

1. At the present time, birth differentials are the result chiefly of the differential use of contraception, and to a very much less extent the result of differences in actual fecundity.[5]

2. Age at marriage is still a major factor in determining size of family—the younger the age at marriage, the larger the family, and this relationship holds among groups who use contraception as much as or more than among groups who do not use contraceptives.[6]

3. Finally, there are indications that among groups who use contraception most consistently and most effectively, the socio-economic and class differentials in births about which we have heard so much in recent years tend to be reversed, with a positive relationship between income and size of family. The evidence on this comes from a great number of studies, no one of which is conclusive, but all of which point in the same direction. A few examples taken at random may be used to illustrate what is apparently an increasing trend.

TABLE 1

PRINCETON MEN

		No. of cases	Per cent married	Av. no. children per man	Av. no. children per married man	Per cent childless couples
Most successful businessmen	(1)	338	93.	1.97	2.14	15.7
Moderate	(2)	366	86.7	1.56	1.80	18.9
Least successful	(3)	54	66.8	1.09	1.64	22.2

The first hint of a reversal of socio-economic birth differentials with an increasing use of birth control came from Sweden. In 1935 Karl Edin, Department Chief in the Swedish Central Statistical Board, published the results of studies made in Stockholm, which showed that size of family in a number of representative groups in that city, particularly in the white collar classes, varied directly instead of inversely with income.[7]

In 1939 J. J. Osborn, in a study of the fertility of Princeton graduates, found a correlation between "Success in Business" [8] and size of family.

The most recent evidence of the change in trends when people practice contraception effectively comes from the study made in Indianapolis on Social and Psychological Factors Affecting Fertility. From the larger study, the "relatively fecund" couples who successfully planned and spaced the number of their children were separated out. The sample was inflated to get a better balance at the extremes. The fertility of these couples in relation to the husband's annual average earnings since marriage was as follows:[9]

TABLE 2

Husband's average annual earnings since marriage	Inflated number of couples	Number of couples	Number of children per 100 couples
$3,000 and over	55	34	149
$2,000–$2,999	94	68	128
$1,600–$1,999	86	81	91
$1,200–$1,599	123	66	97
Under $1,200	44	88	68

[5] Osborn, Frederick, Selective processes in the differential fertility of family stocks, *Amer. Naturalist* **86**: 203, 1952.

[6] Osborn, Frederick, Selective processes in the differential fertility of family stocks, *Amer. Naturalist* **86**: 203, 1952.

[7] Edin, Karl, and Edward Hutchinson, *Studies of differential fertility in Sweden*, London, P. S. King & Son, 1935.

[8] Osborn, John J., Fertility differentials among Princeton alumni, *Jour. Heredity*, 565–567, Dec. 1939.

[9] Whelpton, P. K., and Clyde V. Kiser, Ed., Social and psychological factors affecting fertility, *Milbank Mem. Fund Quart.* **2**: 395, 1950.

None of these studies is in itself conclusive. They cover special groups and include small numbers of people. But they relate to couples who practice birth control more intensely than most of the population, and are therefore to some degree indicative of probable future trends when there are better means of birth control and they are more widely used.

EVOLUTIONARY FORCES IN AN INTERMEDIATE PERIOD

In the first long stage of human evolution the life span was relatively short, a majority died before having children, and births were at or near man's biological capacity for reproduction. Differential deaths were the major factor in natural selection. In the United States this stage lasted well into the nineteenth century.

Today we are probably in a temporary second stage. Among European peoples the death rate is so reduced that an almost insignificant number of people die before the mid-point of their reproductive years, and birth rates are reduced by a wide but unevenly distributed use of birth control. This stage of partial use of birth control is marked by large birth differentials between socio-economic groups. Since we have little or no evidence as to genetic differences underlying the intelligence and personality traits of the different socio-economic groups, there is no way of telling the direction of human evolution with respect to factors for intelligence and personality.

Nor is there any preponderant evidence for the belief that in this intermediate second stage the human race is suffering any dangerous increase in the genes for defect and constitutional weaknesses. We may presume that the heavy losses which take place after conception and before birth have the same effect as in the past. They represent a constant sloughing off of non-viable genes. The 10 per cent of deaths which occur before the age of reproduction, so far as they represent an evolutionary force, are largely concentrated in the first year of life, and must also have the effect of reducing the proportion of injurious genes. We cannot be equally certain of the effect of the present distribution of births; public health measures may result in the survival and reproduction of defectives who without modern medical aid would not have lived to have children. On the other hand, a considerable proportion of defectives are now institutionalized, there is an appreciable use of sterilization, and a few studies indicate that people with chronic or recurrent illnesses now have a relatively smaller proportion of births than in the past. Personally, I doubt whether we need fear an increase in the incidence of genes for defect, and there are some grounds for the hope that in the near future measures taken by the public health authorities with the same sanctions which would be applied to contagious diseases may considerably reduce their incidence.

The third stage would come if new means of controlling fertility should become so effective and widely used that there would be no children born except those who are definitely wanted and planned for. That class differentials would then disappear or be reversed is indicated by the examples of recent studies which we have given above. But at the same time the level of births would be considerably below that needed for replacement of the population, and there would be little chance for the operation of natural selection through a differential birth rate if everyone had small families. Genetic factors for the higher mental and emotional qualities of man could only be improved from one generation to another in a society in which there were marked differences in size of family. Indeed a considerable proportion of large families would be necessary to bring the birth rate up to the replacement level.

FUTURE TRENDS IN NATURAL SELECTION

No one now seriously envisages an authoritarian state which would attempt to decide who should have children and who should not have children. If, as now seems likely, there is developed a cheap oral contraceptive or method of immunization, the birth rate will reflect only the demand for children. Society will have to rely on the free choice of individual couples, both to bring the birth rate up to replacement level, and to provide such a selection of births that the evolution of man's higher qualities will continue and not retrograde.

We have seen some evidence that under such conditions the number of children in a family would bear a direct relation to economic status. Possibly further studies would indicate other social forces which would make for a differential selection of large families, taking advantage of what we already know about the effect of early marriage on family size and other less evident factors. These forces would presumably be under social control. Individual couples would make a less conscious choice, but society itself would be consciously choosing the factors which would play

on the decisions of the individuals. Either way, evolution would no longer be a matter of "natural" selection; it would be a conscious selection by man himself.

SOME PHILOSOPHICAL IMPLICATIONS

We appear to be entering a third stage in the application of evolutionary forces, a stage in which man will consciously guide his own destiny on earth.

There is no time in such a brief paper to develop the philosophical implications of such a finding. A few unanswered questions may provide the best conclusion.

Does the philosopher find it hard to accept the idea of a living species directing its own evolution? Must we again change the basis of our religious thinking?

Will we prepare for such a stage by the necessary research?

Can we in time educate the public to think in the terms of the social scientist, whose predictions are not exact like those of the physicist, but made in the form of probabilities?

Will we be willing to take action on the basis of the knowledge presently in hand, without waiting for scientific counsels of perfection?

Can our western culture survive with only the present proportion of genetically superior individuals, in the face of the preponderant numbers and native vigor of those under dictatorships?

Can man himself rise to decision about his own destiny?

Time will answer these questions. But man will decide whether the answers will be in the affirmative.

AN UNSETTLED PROBLEM IN INTERSTATE DIVORCE LAW

HERBERT F. GOODRICH

Judge, U. S. Court of Appeals, Third Circuit

(*Read November 13, 1952*)

No one disputes any more the proposition that the law grows and that it is not a pre-existing system of rules and principles which give an answer to all the varying problems with which human beings are confronted. We no longer, in other words, postulate what Holmes called a "brooding omnipresence in the sky" to give us the answer to legal problems. The law changes and the law grows. Some people think its growth is far too slow. Others think that the part of the growth which is effected by court decisions sometimes runs too fast. The present president of the American Philosophical Society, then not president but a justice, said one day to his colleagues on the Supreme Court:

> Of course the law may grow to meet changing conditions. I do not advocate slavish adherence to authority where new conditions require new rules of conduct. But this is not such a case. The tendency to disregard precedents in the decision of cases like the present has become so strong in this court of late as, in my view, to shake confidence in the consistency of decision and leave the courts below on an uncharted sea of doubt and difficulty without any confidence that what was said yesterday will hold good tomorrow, . . .[1]

The means by which the law grows, however, are varied, interesting and sometimes as difficult for the laymen to follow as the discussion of some of the biophysicists on the floor of this society. Here is a charming illustration from Blackstone. It has to do with the action of ejectment which was originally one brought by the owner of a leasehold for interference with his possession. This remedy was cheaper, quicker and easier to obtain than that which was available to holders of what were considered more important legal estates than leases. So the problem was how to make this more efficient remedy available to landlords and others. Conceivably it might have been done by a tour de force but English judges are not given to that type of operation even in the interest of procedural reform.

Blackstone describes for us what was done:

> . . . a new and more easy method of trying titles by writ of ejectment, . . . was invented . . . by the lord chief justice Rolle, ... This new method entirely depends upon a string of legal fictions; no actual lease is made, no actual entry by the plaintiff, no actual ouster by the defendant; but all are merely ideal, for the sole purpose of trying the title. To this end, in the proceedings a lease for a term of years is stated to have been made, by him who claims title, to the plaintiff who brings the action, as by John Rogers to Richard Smith, which plaintiff ought to be some real person, and not merely an ideal fictitious one who hath no existence, as is frequently though unwarrantably practiced; it is also stated that Smith the lessee entered; and that the defendant William Stiles, who is called the *casual ejector*, ousted him; for which ouster he brings this action. As soon as this action is called the *casual ejector,* ousted him; for which ration, Stiles, the casual ejector, or defendant, sends a written notice to the tenant in possession of the lands, as George Saunders, informing him of the action brought by Richard Smith, and transmitting him a copy of the declaration; withal assuring him that he, Stiles the defendant, has no title at all to the premises, and shall make no defence; and therefore advising the tenant to appear in court and defend his own title: otherwise he, the casual ejector, will suffer judgment to be had against him; and thereby the actual tenant Saunders will inevitably be turned out of possession. On receipt of this friendly caution, if the tenant in possession does not within a limited time apply to the court to be admitted a defendant in the stead of Stiles, he is supposed to have no right at all; and, upon judgment being had against Stiles the casual ejector, Saunders the real tenant will be turned out of possession by the sheriff.
>
> But, if the tenant in possession applies to be made a defendant, it is allowed him upon this condition; that he enter into a rule of court to confess, at the trial of the cause, three of the four requisites for the maintenance of the plaintiff's action; viz., the *lease* of Rogers the lessor, the *entry* of Smith the plaintiff, and his *ouster* by Saunders himself, now made the defendant instead of Stiles: which requisites being wholly fictitious, should the defendant put the plaintiff to prove them, he must of course be non-suited for want of evidence; but by such stipulated confession

[1] See Mahnich *v.* Southern Steamship Co., 321 U. S. 96 (1944).

of *lease, entry,* and *ouster,* the trial will now stand upon the merits of the *title* only.[2]

The method was cumbersome but it got results, albeit in a slow and lumbering fashion. Instances of this sort could be multiplied. Here is another. Jurisdiction of the English court over an assault and battery which took place in the Island of Minorca was obtained by alleging the fact that this island was in the ward of Cheap, London, an allegation which the defendant was not permitted to deny.[3]

There are other ways in addition to fiction by which our rules of law develop. Sometimes we see growth by the extension of one principle and the subordination of another without an actual repudiation of either.

This statement is a little formidable. It should be clarified by a discussion of the actual problem which it is the purpose of this short paper to present. Here follows the problem and the solution to it thus far.

It has been a rule of law in our Anglo-American system for a good many years that for a state to have authority through its courts to end the bond of marriage between a man and a woman that state must be the domicile of the parties.[4] By domicile is meant the place where the people in fact make their home and presumably spend most of the time. Domicile is a legal concept; it presents some difficulties in application to the facts of life of some people whose careers do not follow routine patterns. But on the whole it is a clear enough concept and can be thought of, in this connection, as equivalent to home. The reason for insisting that only the state of domicile has authority to grant a divorce is that the dissolution of the marriage relationship is considered to be a matter in which the state as well as the individual is concerned. The state has a pecuniary interest in endeavoring to keep people from becoming public charges, and the citizens of the state have a social interest in the kind of family institutions which shall prevail there. So it has been many times said that a divorce granted in a jurisdiction where there is no domicile is a void divorce. This rule has been re-enunciated by the United States Supreme Court no further back than 1945.[5]

A series of cases in that court has brought up in a very interesting way the interplay of this well-settled and established rule and another one which is also well-established and rapidly developing. But first may we ask why the United States Supreme Court has anything to do with divorce law. This is, of course, a matter left to the states under the Constitution. The United States Supreme Court, however, has both responsibility and authority under the Constitution for determining the effect to be given in a second state of a judgment from a first. The reference is to the Constitution, Article IV, Section 1 which provides:

> Section 1. Full Faith and Credit shall be given in each State to the public Acts, Records, and judicial Proceedings of every other State. And the Congress may by general Laws prescribe the Manner in which such Acts, Records and Proceedings shall be proved, and the Effect thereof.

The result of this provision is that a litigant dissatisfied with the effect which a state court has given to the judgment of another can ask the Supreme Court for review and ask with the knowledge that he presents the type of case to which the Supreme Court is particularly sensitive.

Now let us begin our series of cases. We start with one back in 1903.[6] A husband who had been domiciled in Massachusetts began divorce proceedings against his wife in South Dakota. You will remember that South Dakota was once one of the divorce colonies of the country although the industry of the state has now been turned to other things. The absentee wife appeared by counsel and filed pleadings. She was thereby in the litigation. The divorce decree was granted in South Dakota. The husband returned to Massachusetts and subsequently remarried. After his death wife

[2] This is from Book 3 of the Blackstone's Commentaries, Lewis's edition, pages 202 and 203.

[3] Mostyn *v.* Fabrigas, Cowp. 161 (K. B. 1774).

[4] There are interesting questions presented when husband and wife prior to divorce have become separated and established separate homes. Those questions are collateral to the point in this paper. Reference to the multitude of authorities discussing this and other problems on interstate divorce may be found in Goodrich, Eleven years of two-state law, *Syracuse Law Review* **3**: 1, 1951.

[5] Williams *v.* North Carolina, 325 U. S. 226 (1945). An exhaustive analysis of the opinions appears in Powell, And repent at leisure, an inquiry into the unhappy lot of those whom Nevada hath joined together and North Carolina hath put asunder, *Harv. Law Rev.* **58**: 930, 1945. In Rice *v.* Rice, 336 U. S. 674 (1949), *Ill. Law Rev.* **44**: 117, 1949, the Supreme Court sustained the finding of a Connecticut court that a Nevada divorce was not supported by bona fide domicile. See Frumer, The Supreme Court and domicile for the purpose of ex parte divorce jurisdiction, *Syracuse Law Rev.* **1**: 267, 1949; Rheinstein, Domicile as jurisdictional basis of divorce decrees, *Conn. Bar Jour.* **23**: 280, 1949.

[6] Andrews *v.* Andrews, 188 U. S. 14.

one and wife two each claimed administration of his estate as his widow. The Massachusetts court refused to recognize the South Dakota divorce and awarded administration to the first wife. The United States Supreme Court said this was no violation of full faith and credit.

The explanation is as follows: Even under the full faith and credit clause the jurisdiction of a court rendering a judgment may be re-examined in a second state. If there was no jurisdiction, the judgment is ineffective to create rights and the second state is not obliged to give faith and credit to it. Since it was determined by Massachusetts that South Dakota was not the husband's domicile at the time he sought divorce, his decree was ineffective and the fact that the wife participated in the litigation did not change the result.

So far, so good. The result is clear, though it may seem hard. Now comes the second rule. The second rule, which has been developing over the years since 1903, is an application of the principles of res judicata. We call it collateral estoppel. Its general effect is that a party who has litigated a question in one court is precluded thereafter from re-litigating the same question with his opponent or persons close enough to be called in privity with the litigant.[7] The theory behind this rule phrased in Supreme Court language is:

> Courts to determine the rights of parties are an integral part of our system of government. It is just as important that there should be a place to end as that there should be a place to begin litigation. After a party has his day in court, with opportunity to present his evidence and his view of the law, a collateral attack upon the decision as to jurisdiction there rendered merely retries the issue previously determined. There is no reason to expect that the second decision will be more satisfactory than the first.[8]

This principle was first applied in a divorce case in 1938.[9] A husband asserting that he was domiciled in Virginia started divorce proceedings there. The wife appeared in the action to contest the husband's allegation as to domicile. The court found in the husband's favor and granted him a divorce. It was subsequently held by the Supreme Court of the United States that this divorce was entitled to full faith and credit and that he was, therefore, entitled to have a prior separation decree in the District of Columbia modified in his favor.

Note that in this case the parties had fought out the issue of whether the husband's home was really in Virginia. But that was not the situation in the next case of the kind that came before the Supreme Court.[10] In this case a wife went to Florida on vacation and took advantage of its pleasant climate to sue her husband for divorce. While the husband was not served in Florida, he entered an appearance, filed pleadings, and even gave testimony with regard to one element of the case. But he did not contest the question of the wife's domicile in Florida. Later on he sought to convey land in Massachusetts as though he were single. The Supreme Court of Massachusetts refused to let him do so on the theory that the requirement of full faith and credit did not prevent it from re-examining the finding of domicile made by the Florida court and coming to an opposite conclusion.

The Supreme Court of the United States said that would not do. Massachusetts, it said, must give faith and credit to the Florida decree and by that decree the husband and wife were divorced. It pointed out that the husband had appeared in the Florida proceedings, that he had full opportunity to raise any relevant question. If he failed to take advantage of the opportunity afforded him the "responsibility is his own."

Speaking for a majority of the court the Chief Justice got rid of the previous Andrews decision involving the South Dakota-Massachusetts litigation. He said of it, "But insofar as the rule of that case may be said to be inconsistent with the judgment herein announced, it must be regarded as having been superseded by subsequent decisions of this Court. The Andrews case was decided prior to the considerable modern development of the law with respect to finality of jurisdictional findings." May one say in passing that the judicial method of declaring that what one's predecessor has done is either obsolete or incorrect seems to be more gentle than that frequently employed in the academic world.

Suppose, now, that instead of filing pleadings or participating in a litigation in any way, our defendant spouse simply has a lawyer enter an appearance for him. He offers no testimony, he

---

[7] Restatement, Judgments, § 68 "(1) Where a question of fact essential to the judgment is actually litigated and determined by a valid and final judgment, the determination is conclusive between the parties in a subsequent action on a different cause of action, . . ."

[8] Stoll *v.* Gottlieb, 305 U. S. 165, 172 (1938).

[9] Davis *v.* Davis, 305 U. S. 32 (1938).

[10] Sherrer *v.* Sherrer, 334 U. S. 343 (1948). On the same day was decided Coe *v.* Coe, 334 U. S. 378 which presented a similar problem.

files no pleadings. Is he still bound by the result of the litigation so that he cannot subsequently question the jurisdiction of the court? There are several state decisions on this point which say yes, and there is a flat statement of Mr. Justice Reed for the Supreme Court to the effect that "a state . . . must give full faith and credit to an out-of-state divorce by barring either party . . . who has been personally served or who has entered a personal appearance from collaterally attacking the decree." [11]

To what does all this add up? In view of these decisions what will happen if a Pennsylvania man's wife goes to Reno, gets a divorce decree following the entry of an appearance in the litigation by the husband, and then returns to her home in Pennsylvania before her round trip ticket expires? Is she single or is she married? Or is that too simple a statement of the issue? The husband cannot attack the decree on the authority of the cases just cited. Nor can anyone whose relation to him is such as to be called in privity. Who such persons are raises a collateral question which we must not venture into here.

The notion that the state has an interest in the marital affairs of its citizens has certainly taken a hard blow by the application of the rules of collateral estoppel. But it must be noted that what has been said has no application where the migratory spouse has neither served the defendant spouse within the state where suit is brought nor succeeded in getting the defendant to appear in the litigation.

The state of the domicile of these people, however, is not a party to that foreign litigation. The defendant may participate and bar himself from afterwards questioning the validity of the decree, but that estoppel will not run against the state. What risk does that fact present? If the party remarries may he be prosecuted for bigamy or adultery? It is hard to see why not. How great a danger of such prosecution is there? Is this risk a sword of Damocles hanging over the head of the otherwise happy possessor of the migratory divorce decree? Whether the analogy fits depends on how strong the horse's hair was that held up the sword. So far as is known it did not fall. So far as concerns successful divorce litigants, the risk of criminal prosecution is not very great. But it is a possibility.

Will the next step be for the Supreme Court to declare that if the parties cannot complain the public authorities cannot either? To put the question differently: Does this set of cases wherein results have been worked out on the theory of res judicata represent a working toward the social theory that separation and divorce of married people is their business and not that of the public? Or does this series of cases simply represent a spurt of growth in the general principles for bringing an end to litigation? It is easier to ask questions when legal principles are developing than it is to answer them. The same thing is, of course, true of the situation of a man in the middle of a series of experiments in a laboratory. He can see what he has proved so far as he has gone. But probably he cannot be certain of what he is going to be compelled to conclude when the series is over. So here. There is no way of showing from statistics available or surveys or census-takings that the notion that marriage and divorce are affected with a public interest is an obsolete one. It certainly is not obsolete with a very large and vocal group of the population.

The Congress of the United States could settle some of the uncertainties in this field by appropriate legislation under the authority of the full faith and credit clause of the Constitution. There was a bill which looked in this direction in the last Congress. It was not passed. One may suggest that it would be better not to have legislation on the subject until social thinking or feeling on the matter is a little more definitely settled. In the meantime the situation presents an exceedingly interesting phenomenon in the growth of the law.

[11] Johnson *v.* Muelberger, 340 U. S. 581 (1951).

THE PLACE OF DIPLOMATIC HISTORY IN THE STUDY OF INTERNATIONAL RELATIONS DURING THE TWENTIETH CENTURY

SIR LLEWELLYN WOODWARD

Professor at the Institute for Advanced Study, Princeton, and formerly Professor of Modern History in the University of Oxford

(*Read November 14, 1952*)

IT is an honor for me to speak to you in this place, and I hope you will not think that I have taken too much upon myself in venturing to discuss a large subject. I may say at once that my choice of title is not meant to suggest that diplomatic history is a master key to the study of international relations. Diplomatic history deals with the relations between governments, but the reasons why those relations were as they were and not otherwise cannot be understood without enquiry over a much wider field. Diplomatic material is final and indispensable only as an actual record of the transactions between governments.

I must ask, first of all, whether the material at present available is sufficient to allow any general conclusions. I put this question because the archives of the great European States are not open for the twentieth century. The British archives are closed after 1902, the French and Italian archives after 1877. As the years pass, these dates will be moved forward, but I should expect a time lag at least of fifty or sixty years to be maintained. In one important respect, however, there has been a change of policy since 1918. This change indeed began with the publication in 1910[1] of the first volume of a large French collection of documents on *Les Origines diplomatiques de la Guerre de 1870–1*. This publication was entrusted to a committee which included two distinguished French scholars—one of them was M. Aulard. The collection ran ultimately to 29 volumes; it was certainly not produced in haste; the last volume did not appear until 1932, and the fact that a government was publishing its recent archives on so large a scale aroused little interest among historians outside France and Germany.

The First World War brought a new situation. The Allies regarded the Germans as responsible for the war, and assessed this responsibility in terms of guilt. In the main I think that the Allies were right, but unwisely they made no attempt to prove their case by an investigation into the documents recording German policy. The Germans themselves took the initiative and ultimately published a documentary apologia in some fifty volumes. It is worth pointing out that, except in France, these volumes have not even yet been subject to an adequate critical study. The German example was followed by other Powers. Hence the appearance of collections of documents on a scale sufficiently large to enable the student to follow exactly and accurately at least the execution of national policy before 1914. Since the Second World War, in which the assessment of responsibility in terms of guilt was at once stronger and less open to doubt, there has been another spate of diplomatic publications. Moreover, the United States Government has done a good deal towards setting the pace. The official publication of *Papers relating to the Foreign Relations of the United States* had begun long before the twentieth century, but until 1915 this publication had little bearing on the great issues of European policy. From 1916 onwards the American documents were increasingly relevant to European history. In 1939 the volumes published by the State Department had reached the year 1924. This collection has never made the assumption—put forward in the title of the German series *Die Grosse Politik der europäischen Kabinette*—that it is possible to provide from the documents of one government material adequate for describing the policies of all the Great Powers. Nonetheless, as the American volumes increased in number and importance, the European Governments, including the British Government, had to consider whether, at all points where the policy of the United States touched their affairs, they would leave historians to obtain first-hand information solely from American records, or whether they would publish in similar detail their own diplomatic documents for the inter-war period.

[1] The decision to publish the collection was taken in 1910.

There could be no doubt about the answer to this question especially on the British side, though it was clear that the scale of publication would have to be larger than that adopted for the years before 1914. Thus in the British series the documentation of the period from March 1938 to September 1939 will fill nine volumes, each of some 700 or more pages. This material is not, as is sometimes assumed, less authoritative than the material for earlier periods. It is in fact more authoritative because much less business is now done by private correspondence which escapes record in the archives. There is also little in the argument that modern inventions such as the telephone must have affected the completeness of the record. Important telephone conversations are always recorded, and, for obvious reasons, important diplomatic business is not conducted over the telephone except in the form of messages telephoned in cypher. I need hardly remind you of two British instances which show the care to avoid the risks of telephoning *en clair*: one, in Lloyd George's time, when—to elude interception—a conversation from France to England was conducted in Welsh, and the other in 1940 when two high British officers talked in Hindustani.

I might add that, as every student discovers for himself, these large published collections of diplomatic documents in fact cover far more than the execution of policy. It would indeed be surprising if, for example, the instructions to an ambassador did not tell him not merely what he was to do, but why his Government had decided upon such and such a policy; it would also be surprising if, in all matters of high importance, these instructions, and the arguments contained in them, were not approved *verbatim* by the Cabinet or other executive authority of the government sending them. Already therefore there is available for study material comparable in quantity and quality with material at the disposition of students for the study of the transactions between governments in earlier periods.

What is the value of this material to the non-specialist? Has it a practical use? I should not blench overmuch if I came to the conclusion that it had no practical use. It is a form of defeatism to suggest that all serious-minded historians should concentrate upon subjects likely to have a practical use. Furthermore only a rash man would venture to define any period of history as without significance for the present. Nevertheless, here we are—all of us—in the most serious crisis of the world's history, as far as we know it, and if a close scrutiny of what has happened in the immediate past is likely to be even of slight indirect help in saving us from universal calamity, we should be foolish not to undertake it.

The study of diplomatic history can give such help, direct and indirect, though it will never provide automatic solutions of contemporary problems. The direct value of factual information about past policy hardly needs mention. A detailed knowledge of the diplomatic history of the successive partitions of Poland will not solve the problem of the Oder-Neisse line today, but the chances are that a decision taken on the matter without any such knowledge will be wrong. Indirectly, diplomatic material may be of use far outside the field of inter-governmental relations. Thus it may serve the historian of political institutions. Diplomatic material now covers more questions than in the past. The State has thrown its mantle over many matters left until recently to private concern; hence more subjects come to be discussed between government and government. These discussions do not always reach the higher levels of international negotiation, but any one of them may suddenly take a political importance. Apples and tobacco were sharply debated early in 1940 between the State Department and the British Ambassador at Washington; sugar and tea had also had their diplomatic days. One of the most troublesome obstacles in the way of the Anglo-French settlement of 1904 was a question of fish bait and fishing nets. It would indeed be possible to write from the archives of the British Foreign Office a whole book on the diplomatic history of the housewife's larder during the last fourteen years.

I am, however, straying into economic history. My point is that, especially in the larger inter-governmental transactions for which Foreign Offices have become, as it were, clearing houses, the material in the diplomatic records is indispensable for an understanding of the actual working of the machinery of government. Consider, from this angle of view, the history of the British negotiations with the United States over the question of inter-allied debts between 1931 and 1933. On the British side the technical negotiations were conducted mainly by Treasury officials who came for the purpose to Washington. These officials reported, however, through the British Embassy and the Foreign Office. They did so not only because the Embassy and Foreign Office had cy-

phers and a cyphering staff, but for the obvious reason that, while the Treasury officials had the financial *expertise*, the Embassy and Foreign Office had better means of estimating the extent to which political obstacles stood in the way of the kind of settlement which the financial experts would recommend on economic grounds. A study of the relations between these two groups of officials from different departments dealing with the same question thus illustrates the processes by which English affairs are managed. The senior permanent civil servants in the departments concerned were very able men; it has been said with considerable truth that the higher ranks of the English civil service in fact perform many of the duties of an upper House. They belong, broadly speaking, to a common social group; the senior officials of the Foreign Office writing letters to the Treasury official chiefly concerned in the negotiations of 1933 addressed him by the nickname which he had acquired, I think, at Oxford.

There is no short cut to understanding this collaboration; you must plod through the material in the Foreign Office archives. Moreover you cannot understand the general course of British foreign policy at this time unless you follow closely the influence of the Treasury upon the Foreign Office and upon Ministers. The question of inter-allied debts was linked, on the British view, with the question of German reparation payments, but the Treasury and the Foreign Office tended to give different weights to the factors concerned. The Treasury officials (and here one has to remember also the contact between the Bank of England and Whitehall) thought in terms of economic recovery—the restoration of public and private credit and the reestablishment of international trade after the shipwrecks of the great depression. They regarded reparation payment and inter-allied debts as hampering recovery. The Foreign Office officials did not disagree with this diagnosis, but they had to consider the two questions from the political angle and to face the facts that, whatever the British arguments in favour of cancelling reparation, the French electorate thought otherwise, just as, whatever the arguments in favour of cancelling inter-allied debts, the United States electorate thought otherwise. The French were indeed in a dilemna. If German affairs were restored to a condition in which the German Government were able to pay reparation, the Germans would be strong enough to refuse payment, and the nationalist forces in Germany would certainly insist upon refusal. If, on the other hand, German economic recovery were prevented, German nationalism would be less dangerous, but the French would lose all hope of the resumption of reparation payment.

Furthermore French acquiescence in the German refusal to pay reparation meant accepting a revision of the Treaty of Versailles in a matter of great importance, since the justification put forward for these payments was that Germany had been responsible for the war and also for fighting it by methods contrary to accepted international agreement. It had also been clear from 1919 onwards that the Germans were unwilling to tolerate the loss of power which the Treaty of Versailles had inflicted on them, and that they took every concession as a matter of right and as a jumping-off ground for more concessions. Hence, if the German case against the reparation clauses of the treaty were accepted, it would be harder to refuse claims for a revision of the clauses limiting German armaments.

The Foreign Office regarded French tactics as often mistaken. Nevertheless, they realised the strength of the French argument. British policy towards Germany, however, inclined more to the Treasury view than to the Foreign Office view. Ministers were more concerned with the immediate question of British economic recovery than with the remoter problems of the political use which German nationalists would make of German economic recovery. I use the term "Ministers" without party qualification because in those years, although there was bitter party controversy in England, the parties differed little over the question of German reparation. If British policy was wrong—as I thought at the time, and still think—the blame cannot be attributed to one party. The tendency of Ministers to give the main emphasis to the immediate question of economic recovery was reinforced because the Treasury view was put more convincingly, or, at all events, obtained a more careful hearing than the Foreign Office view. This was due, in the last resort, to the vigour of certain personalities as much as to the respective arguments in the case. My point is, therefore, that you cannot fully understand why British policy was as it was, and not otherwise, unless you follow closely the story as it is told in the Foreign Office archives.

I have spoken of the "vigour of certain personalities." Diplomatic records, drawn up carefully and drily by well-trained secretaries, bring

out very clearly the factors of personality and chance in inter-governmental relations. The impersonality of the record is indeed the best artistic background—like the plainest stage scenery—against which the actors can play their parts. I am approaching here one of the questions which historians find it hard to answer, yet I do not think it makes sense to write the history of the relations between governments without giving a large place both to personality and to chance. Stresemann and Hitler were not as far apart as is sometimes assumed in their objectives for Germany, but it would be absurd to suggest that the history of the last thirty years would not have been very different if Hitler's lungs had been affected by his dose of poison gas in the First World War and if Stresemann had lived for four or five years after 1929. Again and again favourable or unfavourable conjunctures of circumstance—one is tempted to risk an astrological term—take meaning out of the patterns elaborated by historians.

Once more there is no short cut, no way other than by reading the diplomatic documents to estimate in each particular case the deflections in policy due to personality and accident. Obviously the diplomatic records must be supplemented from other sources, and obviously they have their limitations. Thus they may leave no evidence of the important part taken by high departmental officials in persuading their political chiefs to accept a line of policy. It may often be necessary to look indirectly for the influence of the chiefs themselves. For example, Arthur Henderson had a definite influence upon British policy during his term of office as Foreign Secretary, yet he has left very little behind him in the records by way of comment or direction. The personal influence is there clearly enough but it is diffused over the whole scene, and an untrained observer might miss it altogether.

One fact of considerable account in the formulation of British policy is never referred to in the documents because it is taken for granted. I mean the location of the British Foreign Office in Downing Street opposite the Prime Minister's house. When the Secretary of State wishes to see the Prime Minister, he has only to walk a few steps across a quiet narrow street. If you think that I am exaggerating the importance of this closeness of access, let me add that a permanent official of high standing regretted to me a short time ago that the proposed new site for the Foreign Office—one of the finest sites in London—was about a quarter of a mile away from the Prime Minister's house.

I have given you English examples; I could quote similar instances from the diplomatic material in other countries of the effect of personality and chance. Thus the volumes of German documents, *Die Grosse Politik,* show the fatal flaw in the German machinery of state before 1914—the confusion of authority at the top. Bismarck had built a system on something wholly ephemeral—the coexistence of a Bismarck and a William I. When neither was there, the system broke down. Or again the personality of the Emperor Francis Joseph was among the reasons for the ossification of Austro-Hungarian policy from 1867 onwards; as one of his Ministers has recorded, almost the only way you could persuade the Emperor to accept a recommendation which he disliked was to be slightly rude to him—impoliteness startled him so much that it threw him off his balance.

It is also possible to see mirrored in diplomatic documents the large impersonal factors, or rather the factors of mass opinion which influence the formulation of policy in countries with parliamentary regimes; the deflections caused by particular interests and pressure-groups, the lobbying in parliament, the over-simplification of issues by the electorate. There is a tendency to regard these factors as actually or potentially dangerous. They are in essence neither new nor confined to democratic regimes—Judaea in the time of Herod and Pontius Pilate was not a democracy. They are not necessarily pathological factors; the many are not always wrong, and the few are not always right. There is also something to be said for taking decisions on broad, simple issues. In any case the historian of foreign policy must make a detailed examination of the diplomatic material if he is to measure the extent of deflection by gusts of popular opinion or the more constant pressure of interested groups. The possibility that Ministers may be called to account is perhaps of greater importance than the actual reversals of policy due to popular disapproval.

On the other hand a study of the records shows that the care of Ministers not to attempt more than public opinion will accept is not always correlated with the amount of public interest in the details of policy. English history in the last hundred years is a good case in point, and, incidentally, provides another example of the effects of chance. From the autumn of 1869 until 1906 it happened that no Foreign Secretary sat in the House of

Commons. To such an extent was the Palmerstonian era forgotten that in 1906 Grey, as Foreign Secretary in the new Liberal Administration, while explaining that he could not find time to come to the House more than twice a week to answer questions, said that it was not in the interests of the House of Commons to accept the view that a Foreign Secretary could not sit in that House.

Parliament itself had become accustomed to leaving the details of policy in the hands of the Cabinet and the Foreign Office. These first months of the Liberal Administration covered the critical period of the Algeciras Conference and, as is commonly said, the testing of the Anglo-French entente. When Parliament met in February, 1906, the Prime Minister made a short reference to the Conference. From this date until the latter part of April the subject was raised only once in the House. A member asked about the progress of the Conference, and the Prime Minister, replying for Grey, said that as the Conference was still in session, he did not wish to discuss it.

This prolonged silence would hardly have been possible after 1918. Mr. Wilson's insistence to the European Powers on "open diplomacy" is a measure of the change of view. Mr. Wilson's argument is well known. He regarded secret military alliances as one of the major causes of war. Such alliances were possible only under a system of secret diplomacy; secret diplomacy was possible only in non-democratic regimes. Mr. Wilson tended at first to confuse secret diplomacy with secret agreements; he never took sufficient account of the risk that his demand for open covenants openly arrived at might lead to the worst combination—secret diplomacy carried on in lobbies under extreme press publicity—though he could not have anticipated the deliberate misuse, for propaganda purposes, of the instruments of open diplomatic negotiation.

There are, of course, certain obvious conclusions to be drawn from the "new style" of diplomacy. It is more difficult both to offer concessions and to withdraw claims if the business of negotiation has to be done in public, or if private discussions are to be revealed in the press while the ink is hardly dry on the *procès-verbal*. I doubt, however, whether the publicity of records bears out the common view of the effects of open diplomacy on the course of policy. It might indeed be said that the field of secrecy has changed, with disconcerting consequences, and that the *arcana imperii* are outside the range of diplomacy and consist of closely guarded mathematical formulae unknown and indeed unintelligible to Foreign Offices. Anyhow, there was a great deal of "open diplomacy" in the nineteenth century. Bismarck, for example, at least after 1870, never concealed his aims; he might be secretive over details, or lie about them, but he was frank and even voluble about his general policy—much franker though not more voluble than the framers of Russian policy today. The change perhaps is not so much that more political secrets are known today, but that the information is spread among many more people. The leakages, or accurate guesses and inferences, which were formerly the gossip of small governing cliques, have become the common property of a hundred commentators in a hundred newspapers. These commentators are read because so many more people are sharply aware of the issues at stake. One can no longer talk lightheartedly about "trouble in the Balkans in the spring."

The issues are of such urgency that my question about the practical value of diplomatic history must mean: can the records be of use in helping us to avoid total war? If we are to look for a pattern in the relationships and negotiations between government and government, our search is primarily in this area. It seems to me that, in the large, one can discern two different lines of approach to the problem of general security. One road leads towards the establishment of a paramount world authority beyond and above the nation-state. Such an authority has never existed. The so-called oecumenical sovereignty of the Roman emperors was not much more than a Mediterranean affair. The paramount authority of the Papacy, "the ghost of the deceased Roman Empire, sitting crowned upon the grave thereof," was not acknowledged by all Christians, and even in western Europe the sphere of this authority was never fully agreed, though it is worth remembering that the noblest of all political tracts—the *De Monarchia* of Dante—was written in advocacy of a just delimitation between the bearers of world spiritual and world secular power.

The rise of the nation state, and the political and religious dissensions of Christendom—not to mention the inruption of the Grand Turk—made it necessary to attempt a different road to peace. If there were no one overriding oecumenical sovereignty, the alternative was an equilibrium of satisfied States—a balance justly devised in which

the territory and standing of every constituent member should correspond, if one may use a modern term, with its "power potentialities." Each State would therefore be reasonably satisfied with its place, and each would have an interest in maintaining the general balance against disturbers.

It is important to notice that in the earlier views of an equilibrium which member States would uphold out of self-interest as well as duty, these States were regarded as forming together a kind of Christan republic. As everyone knows, the principle of equilibrium—acutely manipulated to provide balances within balances—was the leading idea at the three great European settlements of 1648, 1713, and 1814–15. The clearest modern exposition of the doctrine was put forward by the publicist Friedrich von Gentz in 1806. At the time of the Vienna Congress it was still possible to observe the underlying assumption that the European States formed a community, and perhaps one may trace without fancifulness a parallel with the situation of today in the contrast between Alexander's mixture of narrow *Realpolitik* and ideological mysticism and Castlereagh's matter of fact proposal that the leading Powers should meet to discuss critical and dangerous developments.

The idea of a balance of power—a harmonious equilibrium of satisfied States—fitted the contemporary scientific view of a mechanical universe controlled by its Great Architect. The balance was sought for on the assumption that the units in the world of politics, like the stars in their constellations, did not change. You assessed once for all the gravitational pull of each State by taking account of factors which were regarded as constant. The equilibrium was upset, however, not only by the unruly wills and affections of sinful men but by the force of things. Existing factors altered in value, and new factors came in—for example, the acquisition of colonial territory, industrial inventions, the development of oceanic trade. A territory which had seemed adequate now appeared poor because it lacked important mineral deposits or restricted because its millions of square miles did not include "warm water" ports.

The *tempo* of change increased rapidly, and the attempt to maintain a stable equilibrium collapsed even in the restricted European field. The decisive years were between 1859 and 1871; the elements of chance and personality were provided by the *maladresse* of Napoleon III and the genius of Bismarck. About 1875 an English publicist, Henry Reeve, writing, not a political pamphlet, but a sober article in the *Encyclopedia Britannica*, rightly described the balance of power in Europe as completely gone. A year or so later Bismarck himself justified Reeve's diagnosis when he commented on an appeal to European opinion: "Europe; notion géographique."

It might be said that diplomacy abhors a vacuum. At all events the diplomats did their best to produce some kind of a pattern. Between 1879 and 1894 the concept of a balance seemed to be revived. It was, however, a balance which the great exponents of a European equilibrium would have repudiated; that is to say, it was not an equilibrium of satisfied Powers, but a wrestler's hold, a standstill of opposed forces in a contest which still had to be decided. The aim of British diplomacy in the years immediately before the First World War was indeed to keep the situation in suspense; in other words, to maintain the "hold" in which at least the two groups of opponents were doing each other no physical harm, and to hope that by the adjustment of differences as they arose, the strain would be relaxed, and a real equilibrium found.

This attempt also failed, and, with its failure, the system of diplomacy itself fell into discredit. So also did the idea of a balance of power, with the paradox that, when statesmen in 1918–19 tried to establish in the League a community of satisfied States, now on a world-wide basis (it must not be forgotten that the Treaty of Versailles provided for the inclusion of Germany), they explained that they were getting rid of the old conception of a balance of power. They were, in fact, getting rid of the false balance, the wrestler's hold, and returning to something like the conception of their predecessors two or three centuries earlier, a world *respublica* of sovereign states whose collective interests were on the side of peace.

Once more the attempt to provide a framework of peace broke down. The reasons for the breakdown are well known, though there is still much controversy over the emphasis to be attached to any one of them. It is worth mentioning that in and after 1919 public opinion in Great Britain tended to think of the League of Nations in terms of a paramount world authority. If the Covenant of the League had been seen more clearly for what it was—an effort to reestablish an effective equilibrium of national sovereignties—many mistakes might have been avoided. Statesmen would have been more on their guard against the weakest feature in the earlier conception of a balance—that is

to say, the neglect of factors making for change; the public at large might not have fallen into the error of assuming that the League had some *mana*, or compelling power of its own apart from the power resident in its Member States.

What is the practical value of a conclusion of this kind? Perhaps it shows that the study of diplomatic history has nothing more to offer than a certain caution against overhasty condemnation of the past, a certain awareness of the ease with which men of good will fall into the errors of their forefathers. I read as a boy, and have quoted again and again, a sentence written by the English poet and craftsman William Morris: "Men fight and lose the battle, and the thing that they fought for comes about in spite of their defeat, and, when it comes, turns out not to be what they meant, and other men have to fight for what they meant under another name." If this sentence were all, and I would be willing to accept it as the sum of the much-vaunted teaching of history, it is a great deal. We still have before us the two modes of approach to the problem of world security—the attempt at an oecumenical sovereignty—a world government which by definition must be stronger than the strongest national unit—and the attempt to secure from what we have at hand a commonsense equilibrium of nation states. We may be less impressed by the grandiose plan if we conclude from past history that a world government could well be the end of political liberty but that it would not necessarily be the end of war. The diplomats of the older generation would not have supposed that by increasing enormously the prizes of power, they were reducing the temptation to fight for them, or that by facilitating the coercion of minorities they were safeguarding freedom. You remember Jan Masaryk's answer to the question, "what is a free government?" He said that, if he could walk up the main street of Prague, calling out loudly that Czechoslovakia had the worst government in the world, he would know that he was living in a regime of freedom. Who could denounce a world government in this way, and with what result?

We may learn also from diplomatic history that, after all, the nation state, for long the butt of "advanced" political thinkers, is at present the only secular body which secures absolute loyalty from very large numbers, and that, for all its shortcomings, it performs in the world some of the functions of the family in the smaller social community. It is a buffer against competition, and lessens the strains and contests of life. Joseph Conrad, in one of the few passages where he speaks as a Pole, summed up the matter when he talked of the "vulgar refinement" of modern thought which derided patriotism. The closer one's study of the day to day relations between governments, the more clearly does one see the importance of building on what we have now, not on what, for better or worse, we may have centuries hence. The qualities of the professional diplomat at his best have been a sense of limits, a careful analysis of terms, great patience, and strong nerves. The record of action—or of failure to act—on this basis is a contribution which the study of archival material can make to the practical solution of international relations.

There are, obviously, other problems of political philosophy to which this diplomatic material is relevant. I have assumed indeed the answer to the question whether in their mutual relations modern democratic states think and act solely in terms of power. Power is there; it is misleading either to disregard it or to accept it as an *ultima ratio*. It is also most difficult to disentangle it from considerations of right. International law was described nearly a century ago as the measure of conscience of the stronger. The fearful abuse of power in our own time should not blind us to the facts that there is such a measure of conscience and that disregard of it has not been universal. Furthermore, we should remember—and we are justified in remembering with pride—that even in our own tragic time the large-scale abuse of power has been defeated by superior power exercised in the name of right.

Nonetheless a conviction that over the long process of history "God is not mocked," does not solve the problem of justice either for the individual or for the nation in the shorter span of a single human life. "Am I my brother's keeper?" Even when this question is put to a nation in an absolute form, the answer is not a simple one; it is not a fair point against diplomacy that it has translated the parable of the Good Samaritan into the political doctrine of non-intervention. The question is, however, put more often, not in absolute terms, but as a matter of "more" or "less." Thus the acceptance of triumphant wrong [2] may be required as a condition of agreement and collaboration on larger matters. Is there any line of compromise, and, if so, where should this line be drawn? This

[2] This term was used by Lord Palmerston to describe the partitions of Poland.

problem is not a mere academic exercise; it was set sharply to the governments of Great Britain and the United States in their dealings with Russia in the years 1943, 1944 and 1945. It is perhaps—in all its ramifications—the most difficult practical problem today in international relations. The diplomatic material sets out some of the data for a solution. I doubt whether it is possible to extract from these data a convincing answer. One may reject out of hand the impatient conclusion that in the political world bad money must always drive good money out of circulation. As I have said, the records of the past seem to support the opposite view that the full penalty is exacted, without regard to circumstance, for every debasement of the moral currency. One may also reject the idealist answer "fiat justitia et pereat mundus"; our statesmen have neither a mandate to destroy the world nor foreknowledge that justice would abide in general ruin.

What then are we to say? Frankly, I do not know, and because I can give no answer, I may perhaps suggest that the last lesson to be learned from reading diplomatic history—this subject within a subject—is charity to those who have to act, since their decisions cannot await tomorrow, and, in the last resort, cannot be based solely on the wisdom of yesterday.

THE AMERICAN OCCUPATION AND GERMAN EDUCATION

GEORGE N. SHUSTER

President of Hunter College

(*Read by title, November 13, 1952*)

EDUCATION, if we view it aright, is a ceaseless knocking at the door. Young people appear. They are not very different from the countless other young people who in past ages have struggled to grow up. Everything that happens round about them pounds on the drums of their sensibilities with never to be forgotten resonance in the fullest meaning of the term. The language their parents speak sets up a chain of echoes from which they may struggle to escape, while in school and out of it, but which in the long run will have wound itself about their limbs. The longer I live the more convinced I become that the central problem of the educator is this: can the chain be transformed into the materials of a ladder up which the individual may climb to a long-range perception of himself and of the reality which surrounds him? To throw away the past is not the intent of either God or nature. For the past is the only ground on which a human being can stand. He cannot place his feet upon the future because he has no way of telling where the future may be. But if he says that his business is not to reach the unknown that is to be, the spirit of adventure cannot in any way have enlightened him.

It is against the background of some such dynamic philosophy of education that one must place the history of efforts to reform German academic life. For the idea to which the victors originally subscribed was that the legacy of domestic and scholastic training in Germany was wholly evil, since it had produced Nazism. Accordingly, the German past was deemed worthy of destruction. There were several different senses in which the process of demolition was understood. The Russians came out for thoroughly secularized schools which would offer as little resistance as possible to the imbibing of their social and scientific panaceas. The British went in very largely for letting the youth of a dismantled Germany do pretty much what they had always done, subject to rather casual purges of Nazi teachers and materials. The French viewed the situation as made to order for the advertisement of their own cultural achievements, which are very considerable, and so brought to the Germans a great variety of discourses, exhibitions, and concerts which the educated public undoubtedly enjoyed very much. Americans dissented from all these methods. We thought the important thing was to drive the germs of Nazism out of the German system with a variety of wonder drugs, the more important of which has long since purged the United States of all uncleanness.

On the one hand, we were still of the opinion that Russia was a democracy, even if a somewhat crotchety one, and so took a keen interest in demonstrating that we were democratic, too. We, therefore, directed an attack on what was defined as a "class-conscious" educational system. This assault was carried out with such weapons as free text-books for all, insistence that no children should be selected for admission to the gymnasia until they had completed six years of elementary schooling (the standard German practice had been that the weeding out process was to take place at the end of the fourth year), and criticism of the apprentice system. On the other hand, the principal objective was nevertheless the reform of the German character so as to render it immune to the virus of Nazism. Americans believed that the German family was authoritarian, that it was a mistake to segregate children in the interests of denominational religious instruction, that the largest possible amount of schooling was desirable, that co-education should be fostered, and that German teachers should be fashioned into devotees of progressive education doctrine developed in the United States.

It need hardly be indicated that the German who regarded what was being done and said with the requisite measure of seriousness was often more than slightly bewildered. In the first place, he had to reconcile as best he could the very divergent conceptions of what medicine ought to be administered to the Fatherland which prevailed among the victorious powers. Second, when he was not a Nazi, and therefore, not committed to the belief that good schooling was a blend of mili-

taristic ballyhoo and *Mein Kampf*, he was likely to feel that his traditional educational system was not inferior to any other, and that indeed it possessed some special merit. We may also properly note that as the war ended the German was quite persuaded that, unless it were true that the German people wholly lacked political ability and sagacity, it could never have put into power a regime which was guilty both of having created death camps like that at Ausschwitz, and of having brought incredible destruction and suffering to the country's cities and towns. The German was accordingly often psychologically prepared to accept the necessity of *Schulreform*. Such preparation was by no means lacking even in the Ministries of Education, bureaucratic though they might be. But as he became aware of the nature and scope of Occupation plans to alter the character of German schools and universities, he grew increasingly uneasy. And when he then learned of the generalizations which were current among the reformers about the German character and mentality he was often aroused to angry opposition. He had only to look about him in order to observe that the conduct of many who represented the victorious countries was unintelligent and quite immoral. It is not surprising that there should have been formed in the background of his thinking some counter-notions as to how to improve the character of those who, in his opinion, so self-righteously ruled him.

Such was the setting inside which representatives of the United States Government had perforce to carry on. Many of them were able men, and several possessed signal ability. Unfortunately, however, a goodly number were ignorant and mediocre, if not worse. It did not occur to these that any influence they might conceivably exert would depend upon their willingness to learn a little about the character and culture of the people they were supposed to reform. Only a few ventured to make the acquaintance of any Germans. There were officials entrusted with the handling of cultural problems who in seven years never bothered to invite a German to their homes, to look at even the outside of a book published in Germany, or to attend a performance of a German opera or play, excellent though such performances prevailingly were. They sat in their offices and issued orders, frequently reflecting appalling unawareness of the German educational pattern. For these unfortunate aspects of the situation one can blame neither the Army nor the Civilian Administration which followed it. What was demonstrated must be characterized as the inevitable but saddening discovery that the United States was quite unprepared to take on such complex and truly formidable tasks as both idealism and victory imposed upon it.

In my opinion, which is admittedly retrospective, it would have been far better to send over a very few first-rate men and women, to whom there had been allotted freedom to deal with educational and cultural reform in any way upon which they and the emerging pro-democratic Germany could agree. Whenever and wherever this collaboration was attempted by persons equipped to undertake it the results were astonishingly good. Otherwise the Germans gave in if they were compelled to do so, and then, as soon as they felt the absence of compulsion, went back to their own dear old days with badly concealed jubilation. And for our part, many of us began to entertain serious doubts about the unrestricted rightness of our own views. Not a few thoughtful Americans, having carefully examined the school system established to take care of their own dependent children and having compared the intellectual menu available there with what was served to German youngsters, were tempted to believe that their children were the ones who were faring badly. Then Professor Howard Becker came over, surveyed the sociological scene, and threw the talk about the authoritarian German family out the window, and others who looked about and saw that almost the only people really willing to assume risks in opposing Communism were those who had religious convictions, were also left wondering whether the complete secularization of education for the purpose of getting rid of "divisive influences" was such a good idea, after all. In short, the inevitable results of comparative experience began to manifest themselves. No education and no culture has a monopoly of the virtues. One could not any more make mincemeat of the traditional German cultural pattern because Nazism was once triumphant, than one could propose to abolish what is characteristic of the American way of life because political corruption flourishes so mightly amongst us.

As soon as this was belatedly understood, opportunities for fruitful collaboration presented themselves. I should like to list a few of these opportunities as I have seen them. First, the Germans know that political and civic education is imperfectly developed amongst them, and that, the

character of modern living being what it is, the schools at all levels must be prepared to concern themselves with fostering it. Already, partly with assistance from this country, institutes for the study of the social sciences have been established and are beginning to lay the foundation for possible progress. In this respect, Germany suffers alike by reason of its old monarchical traditions and from the partisan virulence which so frequently colors public discussion of important issues. The practice of labor management especially needs to be promoted with studious care. I think I may say that American experience can mean a great deal in so far as these matters are concerned, and that we have every reason to believe that the Germans will avail themselves of opportunities to familiarize themselves with it.

Secondly, let us be frank and say that esthetic and philosophical training is of a better quality with them than it is with us. German elementary schools, for example, foster the practice of and the understanding of music in exemplary fashion. To be sure, this probably means that drill-mastering is still not a thing of the past with them; but I am afraid that I have returned to my country persuaded that trying to teach language without grammar and music without practice may not reflect the highest pedagogical wisdom, if one is at all concerned with the end product. Basic training in the sciences in Germany also reflects methodical care, even though the nation today badly suffers from lack of laboratory facilities without which certain kinds of research cannot be conducted. In short, there are some important values in German experience and practice which can be of considerable moment to us, if we are willing to be objective about the business of education and cultural cooperation.

Third, I should say on the basis of these observations—which could easily be extended into other fields—that a carefully fostered program of exchange can be of immense value, if what has been learned is a criterion. Exchange was at first conceived of by the Occupation as an instrument for smuggling in democratic bacteria. Believing that exposure to life in the United States was the best way of producing carriers and therewith contagion, a very great many people, young and old, were dispatched to this country. As might have been anticipated the results were both good and bad, but I am myself convinced that the good predominated. The bacteria were not scattered about in such profusion as had been hoped, and yet their presence can be easily detected. For instance, there unquestionably is less of the *Kommisstiefel* in the German school than there used to be. German teachers are far more interested in guidance and testing procedures than they once were. German teachers are beginning to be effectively organized. Textbooks have improved, and exude far less chauvinism. What we have now belatedly discovered is that exchange to be effective must be mutual. American students have begun to attend German universities and institutes. Enough of them have now found the experience intellectually profitable to justify the view that going down a two-way street will remove the last vestiges of the curse of one-sided indoctrination.

Then, too, there are creative ideas at work in German academic life which we can endorse all the more heartily because they are born of German tradition and experience. One such idea is that which has established at several universities what is known as the *Studium Generale*. Interested students and professors are banded together within the framework of a residential unit for the purpose of supplementing the specialized training given by the University with courses which promote insight into and discussion of the broader aspects of human culture, so that there will emerge a feeling for the integration of learning. The quality of work done in the *Studium Generale* at such universities as those of Freiburg and Tuebingen is excellent but what impresses one most is the sacrificial willingness of great professors to participate in what is of necessity a pedagogical rather than a research enterprise.

Summing up what has happened since the end of the War, I should say that the results are not spectacular but that they could have been far more meager. We as Americans have talked many things over with other peoples and have concluded that perhaps the mantle of consummate wisdom was not dropped on St. Paul, Minnesota, or Billings, Montana. On the other hand, these have had conversations with us, too, and may have decided that some genuine concern for the advancement of the human spirit is to be encountered even in Spring Hill, Alabama. Today the problem ought not to be whether the inheritors of West European civilization can agree to stop wringing one another's necks, but whether they can in unison bring to the citizen of East Africa or Western Asia some assurance that the blessings, however limited, of technology and metaphysical thought will lift the children of those regions above the

meager levels of physical and mental subsistence to which they have been inured. In short, if we are wise, we shall see to it that such expedients as the exchange of persons between ourselves and Germany do not flatter mere hedonistic desire to bask in the comforts, esthetic and culinary, to which our peoples have attained, but are dedicated to the alleviation throughout the vast reaches of mankind of those ills which breed disillusionment and Communism.

And so I shall conclude by stressing one lesson which abides with me despite all my awareness of what is wrong with Germany and what, indeed, may be imperfect in this country. It is the realization that great men and women residing in Germany have in many a harvest year gleaned insights into the life of Oriental and African peoples which surpass our own. It seems to me that we might well recruit for service, side by side with our own devoted youth, young German scientists and teachers in order to insure the success of what we too prosaically term the Point IV Program. If they can band together for a task which Albert Schweitzer prophetically envisaged when he deserted the playing of Bach on the great organs of Berlin and which is today the crucial endeavor of mankind, then I predict that there never more will be conflict and the aftermath of devastation to sunder our peoples. Perhaps the deepest sin of the Germans derives from an idealism too easily misled. And no doubt also the supreme temptation of Americans is to forget that one cannot exploit sensibilities without at the same time exploiting human beings.

Destiny, if we heed it aright, has associated us in a common and healing enterprise. When a year ago, I visited the University of Erlangen, I joined for an hour students who were listening to a discourse on Plato. The professor assured his audience that they could follow what he was saying with the aid of a Greek text or a translation. The boy who sat next to me, knowing that I was an American professor, nudged his Greek volume over so that I could read from it too, because he would not have thought it possible that a scholar, which he assumed that I was, could be ignorant of that great tongue. And I confess to a feeling of some pride that for once, at least, I was able to uphold the honor of my country. And so you and I can only hope and believe, for the good of the race to which we belong, that when the peoples share the great sources of their common inspiration they will find a comradeship in action and being which may help to bring us peace.

THE MEASUREMENT OF CRIMINALITY IN GEOGRAPHIC AREAS

THORSTEN SELLIN

Professor of Sociology, University of Pennsylvania

(*Read November 13, 1952*)

As readers of newspapers we are regularly confronted with claims that crime, including corruption, bribery, or graft, is increasing. These claims are based on the occurrence of spectacular offenses, official reports issued by a variety of agencies, and statements by persons charged with the enforcement of law. For want of any special knowledge of how to determine the validity of such information most of us probably tend to take it at its face value.

None would question the need or the importance of measurements of the amount and especially the trend of crime. More than a century and a half ago, Jeremy Bentham was looking for such devices in order to estimate the moral health of a nation. A similar hope is probably not completely absent from the minds of present-day moral philosophers. From the point of view of the social scientist, measurements of criminality are essential, because he needs this information for the study of the relationship of such conduct to social change affecting people living in specific geographic areas or belonging to specific social or population groups. The social reformer, in or out of legislatures, needs the same basic information, both to decide when to initiate preventive or remedial measures—and of what kind—and to test the effectiveness of the measures taken.

First of all, what is this criminality that we wish to measure? A legal authority says that "a crime is any act or omission prohibited by public law for the protection of the public, and made punishable by the state in a judicial proceeding in its own name. It is a public wrong, as distinguished from a mere private wrong or civil injury to an individual." [1] Even though violations of municipal ordinances are not included in this definition—there are conflicting legal views on that point—criminality consists then of conduct of all shades of importance prohibited by the criminal law, from treason or murder at the one extreme to disorderly conduct at the other. It includes rare as well as common offenses and those severely as well as those lightly punished. In view of these circumstances, it is obvious that we need measurements of different *kinds* of criminality, rather than of the total mass.

It is assumed that what we want to know are the extent and especially the movement of actual criminality in areas and groups and not merely of criminality that leads to the conviction of an offender. It is also assumed that a crime occurs when a person violates the criminal law, even though none but the violator knows about it. Courts deal only with persons accused of crime. They do so for the purpose of determining whether or not the accused has committed the crime with which he is charged. This procedure chiefly serves the purpose of deciding whether or not the state has any right to punish him in this or that specific way.

Our main inferences about the movement of criminality are drawn from criminal statistics. Certain other sources may have value in connection with some specific offense—such as mortality statistics for estimating deaths due to criminal homicide—but criminal statistics are our chief and indispensable source. By criminal statistics we mean (*a*) uniform data on offenses or offenders expressed in numerical terms; (*b*) derived by official agencies (police, prosecutors, courts, penal institutions, etc.) from their records; (*c*) classified, tabulated and analyzed in order to establish relationships between or among the classes of items tabulated; and (*d*) published—preferably annually—in a uniform manner. Such reports may be issued by a municipal, county, state, or federal agency. They always deal with phenomena in the geographic area over which the agency has jurisdiction. It is important to remember that the statistics contained in them are based on information available in records, which are primarily related to law enforcement activity. They cover only the offenses and offenders that become objects of the attention of public authorities. They are, indeed,

[1] Clark, Wm. L., and Wm. L. Marshall, A treatise on the law of crimes, 2nd ed. by Herschell Bouton Lazell, 1, Chicago, Callaghan & Co., 1912.

only statistics of recorded criminality and recorded offenders.

Since all criminality and all offenders cannot be recorded, those represented in criminal statistics constitute only samples. Our problem, then, is to determine under what circumstances we may assume that these samples are representative of the universe of data from which they are drawn and under what circumstances we may conclude that changes in the size of a sample reflect corresponding changes in its universe. In other words, when can we use the data of criminal statistics as an index of *actual* criminality?

Let us consider, then, some of the problems involved in measuring the frequency of criminal conduct in geographic areas during a specific period of time, such as a year. This is the common period employed in criminal statistics; it permits the development of time series disclosing temporal fluctuations and trends. As a first illustration, we shall take criminal homicide.

All homicides are not forbidden by law. Certain types are either justifiable or excusable. The vast majority of them are, however, banned, but some of them are so successfully performed that they appear to be natural deaths or suicides, while others may be concealed somewhere in the lists of "missing persons." Nevertheless, the high value placed on human life, the strong bonds of blood-relationship or friendship and general inquisitiveness combine to place great hinders in the way of hiding this crime. We have a right to assume, therefore, that the sample of criminal homicides recorded as such is very large. This assumption receives additional support from the statistics of mortality. We must not forget, of course, that the determination of the cause of death is a technical problem and that, in our country at least, the antiquated coroner system, especially in rural areas, introduces a variable which may be important. Nevertheless, it is likely that recorded criminal homicides afford a good basis for the measurement of the incidence and the trends of such conduct in an area. This type of criminality is highly reportable, in spite of the fact that the chief victim of the crime has been removed.

We might divide all other crimes into two classes, merely for the purpose at hand. One of these classes contains crimes in which there is a private victim, either an individual or a private enterprise, the other class consists of those offenses which can be said to be against public order or policy. In the first class, we find offenses that cause injury either to the person or his property. They are offenses in which the victim is hostile to the offender and therefore is to some degree eager for his punishment with the result that he is more or less likely to report the crime to the authorities. The reportability of such crimes varies with their nature and the amount of injury inflicted; it may even vary in different social groups within the area. It is reasonable to assume, however, that the recorded samples of crimes like robbery and burglary, serious assaults and batteries, false imprisonment, rape by violence, and thefts of property or money above a certain value, would be large enough to permit us to use them as a basis for measurement. Certain varieties of offenses are probably recorded in every instance—bank robberies, for example.

On the other hand, there are offenses in this class which are so rarely brought to the attention of the authorities that the recorded sample is infinitesimal or at least so small that we cannot consider it adequate for measurement purposes. Chief among them is the larceny of objects or money of little value. There are no doubt others, for instance, blackmail, but there has been so little research done on these questions that it would be unwise to be too categorical.

The second class of offenses referred to above are those in which public order, decency, morality, or policy might be said to be the sole victim. These offenses could no doubt be classified in various ways. For the sake of convenience, we shall distinguish only two sub-classes here. In the first of these we place crimes of a clandestine nature in which the "victim"—if that term is appropriate—assists the offender in concealing the crime or participates in it. Practically all sex offenses belong here, such as statutory rape, incest, sodomy, fornication, adultery, illicit cohabitation, etc. We might also add bribery, bigamy, and abortion.

In the second sub-class we might place all offenses against the administration of justice and government (except breaking prison, rescue of prisoner and disobedience of lawful order, which are either fully or adequately known to the authorities); all offenses against public safety, health, and comfort (especially disorderly conduct); all offenses against public trade; and crimes like public drunkenness, vagrancy, gambling, prostitution, carrying concealed and deadly weapons, receiving stolen goods, etc. These lists contain only illustrations.

Most offenses in the above two sub-classes have one thing in common. They are almost never reported to the authorities by anybody outside the police forces. As a rule they become recorded only in connection with arrests. The police may actually know of some of them, such as the existence of bawdy houses, gambling dens, etc., but a record is made only when an arrest occurs or a summons is issued. This means that in those offenses, the only thing measurable by the recorded criminality is the extent and the trend of the activity of law enforcement agencies. If the police become more active more offenses are recorded and *vice versa*.

We arrive, then, at the general conclusion that measurements of *actual* or real criminality in a given area during a specific period of time can probably be made on the basis of *recorded* criminality, but only for certain kinds, in which the recorded sample is large enough to warrant the assumption that changes in its dimensions reflect proportional changes in the unrecorded criminality. The crimes most likely to meet these specifications are those which arouse the greatest public concern, are least surrounded by secrecy and cause the victim or those near to him to appeal to the authorities for aid in bringing the offender to justice.

Now, even if the above analysis is valid, the question might be raised as to what series of recorded criminality would be most adequate for our purpose. Inferences concerning the movement of actual criminality are usually based on the criminality recorded by the police or on that recorded by courts. Police records contain information on crimes reported to the police and upon investigation found to have really occurred, whether or not the offender is known. They also contain information on the crimes discovered by the police themselves in the course of their tours of duty, and data on arrests made, with or without warrants. We could, therefore, construct measurements of this or that (*a*) crime "known to the police" or of such (*b*) crimes leading to arrests. From court records we might construct measurements of (*c*) crimes resulting in the prosecution of a suspect and (*d*) of those leading to his conviction. Additional series might also be set up but those mentioned are sufficient for our analysis.

It is frequently assumed that the best basis for inferences concerning the trend of criminality is found in the records of convictions. This assumption has not been seriously challenged until the last two or three decades. We now have very good reasons for believing that even in the case of the few kinds of criminal conduct which can be measured by the aid of criminal statistics, the recorded convictions are not adequate and that the records of crimes "known to the police" offer greater possibilities of usefulness. A jurist might, of course, argue against this assumption by saying that police authorities lack the legal training to determine whether a crime they are investigating falls in this or that category or is a crime at all, and that in most instances perhaps these details cannot be determined without catching the culprit and examining his state of mind and his motives. There is, of course, a great deal to justify that position. On the other hand, the kinds of criminal conduct, such as burglary, aggravated assault, forcible rape, and robbery, which can probably be measured by criminal statistics are of such a character that they can be identified in most cases by a skilled investigator without extensive legal training. As for criminal homicide, it would be difficult perhaps to determine merely from the circumstances surrounding the crime, whether or not it is first degree murder, let us say, but if we are satisfied to know only whether it was due to negligence or not, the determination can probably be made by the police within a narrow range of probable error. It must be recalled that in view of the strange behavior of juries in many cases, what the legally trained prosecutor has decided is one kind of crime may prove to be something different when the verdict is returned, and that the practice of accepting pleas of guilty to lesser offenses in order to expedite justice makes the record of convictions of doubtful value for some purposes. In other words, there is no reason to think that the "crimes known to the police," of the kinds in which we are concerned for purposes of measurement, are not as well catalogued by the police as are those subjected to the procedure of the courts. We assume, naturally, that we are dealing with police data which are honestly recorded.

If there were any doubt about the superiority of the "crimes known" over other series, a recent report issued by the Belgian National Institute of Statistics (corresponding to our Bureau of the Census) should dispel it. This investigation [2] deals with rates of crimes (corresponding to our felonies and misdemeanors) leading to the conviction of an

[2] Ledent, R., La criminalité impunie en Belgique, *Bull. de Statistique*, nos. 7-9, July-September, 1951.

offender and crimes which have been investigated and identified by the prosecutor's office but have not been subject to prosecution because the offender was unknown. These two groups, the punished and the unpunished crimes, represent the total criminality known to the public authorities. The statistics available for analysis covered the period 1871–1949, a series of such length and importance that it does not have its counterpart in our country. We shall use as illustration only one group of crimes, namely, those against the person, which in the study include murder and voluntary manslaughter, infanticide and aggravated assault, including assaults resulting in the death of a victim. During the five-year period 1871–1875, the annual average number of such offenses leading to convictions was 69, while the average unpunished number was 62. In 1949, 54 of those crimes led to conviction, while 563 went unpunished. In 1944, the corresponding figures were 17 and 1,411. Any one using the Belgian conviction series for measurement purposes would conclude that these crimes have shown a generally downward trend, at least since 1895, while any one using the data on the *total* criminality would conclude that they have shown a general rise until they are two and a half times as frequent in 1949 as in 1871–1875, taking into account the growth of the population. It is obvious that in Belgium it is necessary to employ the figures on "crimes known," whether punished or unpunished, if one is to get any sound basis for measurement. There is no ground for believing that the situation is different in our country or any other country. The reasons need not be discussed here, but we are compelled to draw the conclusion that statistical series of crimes resulting in arrest, prosecution, conviction, or commitment to prison on sentence are not useful for the measurement of criminality in an area. It behooves us, therefore, to make sure that police authorities that investigate crimes committed develop good records so that we will get the basic information needed for adequate measurement.

There should be no need to point out that even when we have discovered the necessary absolute figures of those crimes known to the police which can be used for inferences with regard to the frequency of such conduct in an area, we must reduce these figures to rates in order to eliminate the effect of changes in the size of the population over a period of time. It is obvious that great increases in a population will bring with it increases in criminality. The question is whether the latter increase is disproportionate. There are numerous technical problems in this connection that cannot be discussed here. They are merely suggested by the following question: Are crude rates computed for the total population good enough, when we know that ninety per cent of the crimes, let us say, are committed by young males, when the crimes we are interested in are almost never committed by children, and when sex and age ratios may change greatly in an area over a period of time? In the United States, we are especially handicapped in developing good rates, because of the absence of any real population accounting system during inter-censal years. This has caused a ludicrous situation to develop in connection with the much publicized statistics of crimes known to the police issued by the Federal Bureau of Investigations. These statistics, gathered regularly from municipal police departments in all parts of the nation, are well planned and furnish us with most important information with regard to the number of several important crimes committed in urban areas, but in addition to giving us gross figures, the Bureau also computes rates per 100,000 population for individual states and regions. Because these rates are based on the population in the municipalities which report crimes to the Bureau and because it is impossible to secure population figures for local areas in inter-censal years, the Bureau keeps on using the figures of the latest census until a new one is made. The effect of this procedure is to make the annual crime rates published by the Bureau in its semi-annual *Uniform Crime Reports* more and more inaccurate as the inter-censal years go by. For instance, during the decade of 1940–1950, the population of the three Pacific Coast states increased about forty per cent, but in 1949 the crime rates of the municipalities in these states were still being computed on the basis of their 1940 population, which means that the crimes of 1949 were charged to only sixty per cent of the people of those states rather than to the entire population. The crime rates would have been much lower if they had been correctly computed.[3]

[3] For California, for instance, the effect of this procedure in computing crime rates per 100,000 population can be seen when we compare the rates for 1940 and 1949, based on the 1940 census, with the rates for 1950 and 1951, based on the 1950 census. The extreme differences between the rates of 1949 and 1950 reflect the change in the population basis for the rates rather than a change in the frequency of the crimes involved.

In states or regions which show a population decline during a census decade, the rates distort the picture in the opposite direction.

The above illustration shows how difficult it is to arrive even at crude crime rates in a country like ours, which has a very mobile as well as a growing population but possesses no means of keeping current track of population movements in and out of states and local areas. We shall probably never be able to develop as satisfactory measures of criminality as those of many European countries, for instance, which have not only smaller populations but often keep a much more adequate current check on population changes.

The criminal statistician sometimes makes changes in crime rates over a period of time easier to visualize by using a procedure which is employed in the Belgian study referred to. The author of that article gave to the annual average rates for the 1871–1875 period the base value of one hundred and translated the rates of all later years into ratios that showed percentage changes in terms of the base value. The problem is to decide what time period, usually a five-year one, should be used to establish the base of the index. If a period of abnormally high criminality were used, later changes would appear as favorable, and *vice versa*.

Crime	1940	1949	1950	1951
Murder and non-negligent manslaughter	4.1	4.97	3.21	3.50
Aggravated assault	34.3	95.4	69.7	57.1
Robbery	83.0	136.1	84.5	85.6
Larceny-theft	1509.2	2141.6	1590.5	1669.4
Burglary	507.7	756.8	522.4	523.5

So far as is known, only one person, Professor Diego Di Castro of the University of Turin, has in recent years proposed the development of a composite index to criminality similar in nature to a cost of living index. He would give different weights to the crimes included in the index, the weighting being determined by the relative seriousness of the offense as judged by the penalty attached to it in the penal code.[4] It is an interesting idea, but has not yet received any official attention. Furthermore, the theoretical aspects of the problem have not been adequately analyzed.

We have dealt with only one phase of the measurement of criminality, namely, criminality in the total population of geographic areas. Unfortunately, the time at our disposal does not permit us to pursue the important problems involved in measuring the criminality of specific population or social groups, such as age, sex, race, nativity, occupational, and other groups.[5] Many of the observations already made would apply here as well, but there are in addition a number of special problems which arise out of the different roles and functions of these groups in our society, and they increase the difficulties we have already found facing us when we try to fix the dimensions of criminality.

[4] Metodi per calcolare gli indici della criminalità, Torino, Istit. Giuridico della R. Università, 1934.

[5] For a fuller discussion see Sellin, Thorsten, The significance of the records of crime, Fourth annual lecture in criminal science, Cambridge University, 1951, *Law Quart. Rev.* **67**: 489–504, Oct., 1951.

ON THE NATURE OF SCIENTIFIC RESEARCH

DAVID MEREDITH SEARES WATSON

Professor, Harvard College and University College London

(*Read November 13, 1952*)

FORTY-FIVE years ago, when I had just graduated as a geologist, I had to consider what I would do and how I should make my living. The only obvious posts were in government service, in the Geological Survey or British Museum, or in a University. Actually I was offered a post as an oilfield man in the district of Baku, the first beginning of what has since become an immense field of employment.

During the years since the end of the late war I have interviewed some hundreds of young men and women who wished to become zoologists. I asked each of them how they hoped to earn their living and the great majority said "I want to do Research."

The assumption that scientific research was a livelihood like any other, which anyone could do and be paid for doing, seemed and still seems to me extraordinary, though in fact the British and all other governments advertise for officers to hold posts in research institutes, and in fact employ them in great numbers.

Indeed I suppose that everyone in the vast hierarchy of the employees of the "Atomic" research institute is a research worker, in the modern acceptance of the term.

But this development of work under the direction of scientific men tends to obscure the real nature of scientific research, confusing it with much *ad hoc* investigation, degrading its meaning.

It thus seems to me that it may be useful to explain what scientific research means to me, and, I think, to most men of my generation.

I may perhaps be forgiven for recounting a personal experience, for it is only so that any real understanding of scientific method can be gained.

After my graduation in Manchester in 1907 I remained at that University with a scholarship followed by a fellowship and an appointment to the staff.

An accident, the gift of some fragments of coal-balls to me as a school boy by a man who knew only that I had some interest in fossils, turned my attention to fossil plants and as I had learned the technique of cutting rock sections I naturally made sections of these coal-balls. I could not find anything about what I saw in them but turned to the Botanical Department where, at the end of my first term at a University, Professor F. E. Weiss and Dr. Marie Stopes found that my sections contained seeds of a kind which had just been described by Drs. Oliver and Scott and shown by them to have been borne on fern-like plants. The general wide interest in "fossil botany" resulting from this and other new discoveries led me to study fossil plants and I then decided to work on the club mosses of the Coal Measures called Lepidodendron, and became very familiar with these plants, both in their external appearance as seen in impressions, and in their structure in sections of petrified materials.

Soon after the first discovery of these lycopods it was recognised that there was in them a condition, found rarely and only in large trunks, in which the series of rhomboidal leaf-bases which regularly covered the outer surface of the stem was interrupted by the presence of two opposite vertical rows of great scars. These were either circular and so closely set that they nearly touched one another, or were oval, vertically elongated, and widely separate.

Each scar bears a definite circular area of small size, which may be central or, in oval scars only, may lie toward the lower end of the scar.

This circular area is surrounded by many small scars spirally arranged, usually elongated and directed outward from the center, which are restricted to the scar area.

Their original discoverer called such trunks Ulodendron, with an added trivial name.

For nearly a hundred years the significance of Ulodendron was discussed. Was it really a plant differing from Lepidodendron, Sigillaria, and Bothrodendron, genera established on the character of the leaf bases, or was it a special condition of one or other or all of these?

If it were a special condition what lateral struc-

ture was attached to the scars: was it a reproductive cone, or a bulbil, or an adventitious root? The whole problem produced a considerable literature and was discussed for very many years.

In 1880 D'Arcy W. Thompson, later the author of *Growth and Form*, found a specimen which he believed to show a cone in actual continuity with the central ring of a ulodendroid scar, and in 1885 R. Kidston wrote a paper on Ulodendron and an allied condition called Halonia in which he was generally believed to have solved all the main problems of the group. In this paper D'Arcy Thompson's specimen and one or two more served as test pieces, and the ultimate conclusion was that the ulodendroid condition occurred in Lepidodendron and Bothrodendron and that its character depended on the attachment of a sessile cone to the central ring alone, the greater part of the scar representing an area of leaf bases crushed by the base of the cone.

For some time this conclusion troubled me; it did not seem reasonable to assume that the genus Bothrodendron should have in one species cones about a centimetre in diameter and more than ten long attached to the ends of slender twigs, and in another minute cones only some three millimetres wide by ten high, whilst a third should have great structures with a base perhaps ten or more centitres in diameter, completely sessile on the main trunk.

But I could never see the explanation of the problem until one day I went into the Museum belonging to the University in order to pick out some fossils for use with a class, and by pure accident saw in a case I passed a specimen of Ulodendron which I had known for some years, and had always felt to be of little value.

This specimen now, instantaneously, showed me the real solution of the Ulodendron problem.

I unlocked the case, took the specimen to my room, photographed it and wrote a paper about it in perhaps a single day. This was sent to the Manchester Literary and Philosophical Society, read at a meeting and published a few days later!

The explanation was that the scar represents the place to which a relatively small side branch was attached to the trunk, the central circular area is the broken off wood of the branch, the small scars arranged in spirals are intersections of vascular bundles on their way to the leaves which covered the branch, and the definite smooth general surface of the scar could only be that of an abscissive layer newly formed, similar to that which produces leaf-fall in the broad leaved trees of temperate climates and leads to the shedding of branches in the conifer Agathis.

Most of the established palaeobotanists received this view with much hesitation and dislike, but about a year later Dr. Renier of Brussels found a specimen in which a branch was actually attached to a ulodendroid scar. But he claimed that the attachment was only to the central area and that by growth the base of the branch had become buried in a conical cup in the bark of the trunk.

I had therefore to accept the confirmation of my view of the nature of the appendage and to discuss in detail the nature of the attachment. This presented no difficulty, petrified materials showed the real existence of such a branch-shedding mechanism in lepidodendroids, and the specific pieces of evidence adduced by Dr. Renier were easily shown to be capable of other interpretation. Finally, a couple of years after my second paper, a complete specimen of a most curious lepidodendroid side branch widening out abruptly to the counterpart of a ulodendroid scar was found and described. Since then my interpretation has, I believe, been universally accepted.

This particular problem is a small one of no general importance, but it remained unsolved for nearly a hundred years. Its solution was an instantaneous event, an enlightenment, in my mind. And I believe that something of this nature lies at the back of all real scientific discovery.

In the particular case I have described the possibility of the solution depended on the fact that I had seen much material, was familiar with the structure of lepidodendroid stems from twigs two or three millimetres in diameter to trunks 30–40 cm. across; I had seen dozens of specimens of Ulodendron and great numbers of cones.

That background of knowledge was all immediately available. I could call up into my mind at any time a particular specimen and there compare it with another or with a whole series. In fact, at that time lepidodendroid problems and materials were intermittently but constantly present in my mind. And on the occasion of this event the specimen accidentally seen came for the first time in my mind into a particular complex of facts, memories of other specimens, speculations as to modes of growth, the possibility of seasonal variations, and so on which made its interpretation obvious. And so it is treated in my first paper.

But M. Renier's alternative explanation required different treatment. I was at once con-

vinced that his interpretation was impossible, but to show that it was so meant that I had to recall the existence of specimens which might give new evidence on the matter as well as to see all those which had already been figured or publicly referred to. And this involved a good deal of traveling about, the critical examination of very many specimens, a search through all the available sections of lepidodendroid stems with branches, and a renewed survey of the literature. This took much time, for each specimen had to be examined to see whether it showed anything "new," different to things commonly shown, which might throw new light on the condition.

In fact I learnt from this study that my original interpretation was correct, and the further fact that ulodendroid trunks very seldom if ever showed definite leaf scars on the leaf bases. When whole branches were shed it was unnecessary to shed each leaf individually, as normal lepidodendroids did.

All subsequent experience has led me to believe that the essential element in scientific thinking is the coming together in the mind of a series of "facts" which fit together to give what is immediately felt to be a convincing explanation of some problem.

The interpretation so reached must then be submitted to criticism. It must be brought into relation to other "facts" which have to be called into a conscious mind either from memory direct or from some source whose existence is remembered. In all experimental sciences it is natural to make predictions as to events unknown which should occur under defined circumstances if the conclusions in question be in fact justified, and to see whether they do so occur. In many sciences (geology for example) experiment is impossible, but "proof" in them does not differ in its nature from that by experiment, though it will usually lack quantitative accuracy.

Experimental work sometimes means envisaging a particular type of experiment, which involves the design and manufacture of special apparatus. This may be a lengthy and probably a delightful process; it may take six months to make and test a galvanometer, and ten minutes to do the actual experiment with it!

In any case there are necessarily several consecutive stages.

The worker must have learnt much, though the nature of what he has learnt varies greatly in different sciences. In systematic zoology, a very old study now revivified by new directions of approach, the essential basis is a detailed knowledge of the structure, habits, and life histories of a large number of animals which fall within a group. This knowledge can come only from study not only of actual animals but of the literature of morphology, and its generalisations gain their present interest because they are part of the raw materials of evolutionary studies, and are thus of interest to geneticists; and in return an indication of their possible meaning comes from the facts of that science.

Physics (whose nature I make no pretense to understand) is a study in which events, though their first recognition, as for example that of the α particles of radium by the "spinthariscope," is of interest to physicists, cannot really be understood until they are reduced to quantities, related to theoretical views of the nature of the Universe, and expressed in mathematical equations.

I can well remember an occasion at the Royal Society in London when Dr. Eddington gave a short account of "relativity." In effect he wrote across a blackboard a great equation, smiled at us in a friendly way and said, "That, gentlemen, is Einstein's theory." J. J. Thompson was in the chair; he looked long at it and said, "Can Dr. Eddington tell us what it means—in English!"

It is difficult to compare two such immensely different sciences as physics and geology. But in essence I think the process of discovery is the same in each.

There is an initial period of learning facts, and putting them into simple groupings. A problem comes into the mind of the student from consideration of these facts. I assume that in every active scientific mind facts within its field of interest are constantly coming into and going out of consciousness, and all kinds of problems present themselves. Some may be, without deliberation, picked out so that they come to mind time after time: until at last they come into a context which gives a key to their solution.

Once this has been discovered, it has to be tested, by a trial with facts already known, or by facts in a field which can be envisaged, within which they must be sought from actual materials if possible, or from the "literature."

In such a hunt innumerable new facts, not needed for the immediate investigation, come to knowledge and may at any time enter into new comparisons; so that the process goes on indefinitely.

But all this means that the basis of the whole process is memory. It is facts in the mind which

are brought together. Facts in books mean nothing until by being read they form part of a mind's stock, and only when so available can they be used. As Isaac Newton implied scientific discovery really depends on an ability "to hold a great number of different matters in the mind together for long periods."

This view of the nature of scientific discovery implies that it is a purely individual matter. Facts must come into an individual mind, not be dispersed amongst a dozen.

This raises the problem of "joint" work. Of such work perhaps the most distinguished was that of the physiologists Bayliss and Starling. These men worked together for some forty years, and the work which they did was fundamental. Many branches of physiology spring directly from it. And it was really joint work. Both authors did work of first class quality separately. Each of them was a man of outstanding ability. And they differed very greatly in personality, in general behaviour in ordinary life, and I believe in the texture of their minds.

But they lived in close association, and must most evidently have discussed matters together so constantly that their minds were often engaged on the same problem at the same time, so that in effect they could pool their memories of possibly relevant matters.

But there is all the difference in the world between two men, equals, and the list of A–Z it is now proper to present as the authors of some piece of scientific work.

I do not know how generally other men who have experience of scientific discovery will feel that my account of the process conforms to their own personal experience, though I expect that a good many will do so.

But if the process be generally as I have described it, it is important to realise that it corresponds closely to that involved in the practice of the fine arts.

Two great artists, Albert Dürer and Leonardo da Vinci, have left us great series of drawings, more than a thousand in each case, which represent their interests over the whole period of their active life. Consideration of these drawings shows that each of these men was continuously viewing everything around him, fascinated by appearances (facts) of every kind, and immediately recorded anything of interest in a drawing. Though Leonardo, as his notebooks show, made many drawings not as casual records, but as part of elaborate investigations of specific subjects. They were men of immense curiosity, as any scientific man necessarily is, and their minds must have been stored with "facts," not only with visual appearances, but with neo-platonic or other symbolic reflections of these facts.

Thus it would be natural that schemes for the construction of future pictures or statues should be constantly present in their minds, there brought into association with the stores of memory and ultimately perhaps carried into effect.

In the case of Leonardo and of Michelangelo we have what are perhaps the first beginnings of works of art. Leonardo in an anatomical manuscript makes a scribble about two inches square which is a design for a picture of Leda and the Swan. Michelangelo on a larger scale sets out in the most rudimentary way a scheme for the Medici tombs, which very long after were erected in a form much simpler than, though including the elements of, the original sketch.

These sketches, records of possible forms of a picture, serve merely to record a composition; the process of converting them into a finished work involved the making of many drawings from models, often the use of earlier made drawings, and finally the drafting of a full size cartoon.

Only then is the final canvas embarked upon, or the marble of the statue attacked.

Once complete and exhibited the picture, if by a great master, serves as a model to lesser men, it is copied with variation by the master's workshop, it provides a scheme of composition which may be used for pictures sometimes of quite different subjects.

It may start a fashion which lasts, sometimes for hundreds of years.

But it was in origin, even where the execution was largely the work of assistants, the product of a single mind.

I do not think I need labor the comparison of such a process with that of scientific discovery; it is evident that the two are in essence similar. And confirmation of this view is given by the fact that the work of the greater scientific workers, and even of smaller men, has an individual style which allows a part, even only a paragraph, of a good scientific paper to be referred to its author even if read in translation. The style is a matter, not of words, but of the character of a mind.

If this account of the nature of scientific research be true, it must be kept firmly in mind in the design of research institutes and of universities.

It seems to me evident that much of what now passes for scientific research does not get beyond the level of the collection of data.

How far such work is necessary or even useful varies greatly. For example the search for new insecticides goes on continually in the "research laboratories" of the great chemical manufacturers. These places maintain stocks of insects in culture, so that on any day they can produce samples, of statistically significant size, of a series of developmental stages of a number of insects on which the possible toxicity of any substance may be tested. They go on day after day trying substance after substance. Our knowledge of such matters suggests kinds of substances which might, from some feature of their molecular structure, be expected to be toxic. And if one be found which is promising, then all kinds of modifications of it are made and tested. So they go on collecting "data" until something satisfactory presents itself. Then there is a long period of trial of the substance under all kinds of practical conditions, all over the world. Then it may fail, but it may affect human life on the largest scale, as the attacks on malaria in Cyprus and in Ceylon have done.

But such work is not I think truly described as scientific research. It is an investigation designed to build up a stock of facts. It is valuable work. The discovery of new antibiotics must be a very similar process, but it does not necessarily contribute at all to scientific theory. And it is the development of theory which is the most important matter, not only to scientific men but to the world in general.

And in that such developments come from the minds of individual men, it is necessary that such persons should be sought out and supported. Their living costs are low, the expenses of the researches may usually be infinitesimal compared with the costs of the accumulations of data necessary for the development of industrial processes.

But it seems now to be more easy to finance a great "project," meaning the employment of many men and a vast expenditure on equipment, than to obtain the relatively infinitesimal amount needed to support a single able man in work which may involve only very minor expenses. Some industrial firms have in fact done so, and to do so seems to me the special duty of the universities.

And the discovery of the men themselves can only be done by those who teach them in their early stages, that is again by universities.

THE FRIENDSHIP OF THE "REFORM" CARDINALS IN ITALY WITH DAMIÃO DE GOES*

ELISABETH FEIST HIRSCH

Assistant Professor of Philosophy, Bard College

WHEN Damião de Goes stayed with Erasmus in Freiburg he must have felt that his life had reached a climax. Unfortunately his residence had to be cut short because of his Protestant leanings, which aroused indignation in Freiburg.[1] Where should he continue his studies? The choice fell on Padua and without doubt Erasmus was responsible for the decision. Why did Damião de Goes not return to Louvain, from which he had come when he visited Erasmus and to which he would return after four years in Italy? We know how much he loved Flanders, where he would have liked to stay for the rest of his life.[2] It is more than likely that Erasmus had warned his Portuguese friend not to return to Louvain, which the great Dutch humanist had left once because of its Catholic orthodoxy. Damião de Goes would have run into similar trouble as in Freiburg.[3]

It is quite interesting to note that the religious atmosphere in Italy at the time when Goes came there was much more open and liberal than in any of the lands belonging to the domain of the Emperor. When Goes arrived in Padua in the Fall of the year 1534, Alexander Farnese had just ascended the Papal throne as Paul III.[4] The years between 1534 and 1541 were distinguished by consistent efforts on the side of the Pope to organize a general Council in which religious questions should be discussed with the Protestants.[5] High ranking members of the Catholic Hierarchy read and discussed Luther's and Melanchthon's religious writings.[6] It was also the time when Juan Valdès talked to a group of friends in Naples about his faith in the "inner light."[7] Ochino and Vermigli, his pupils, preached to Catholic audiences explaining their "spiritual" concept of religion. Both were to become prominent Protestants.[8] The poetess Vittoria Colonna, Marquise of Pescara, protected Ochino, who had already been called to Rome in 1536 to defend his religious position.[9] In addition, German merchants introduced Protestant literature into the north of Italy. In Venice, which Goes visited frequently, he must have been reminded of Antwerp, where he had been present at many discussions of Lutheran ideas at the beginning of his career.[10] Paul III called into his cabinet men like Bembo, Sadoleto, and Pole who had many connections with the "evangelical" movement in Italy and who made at the same time sincere efforts for a Catholic reform.[11]

* The research for the article was made possible by a grant from the American Philosophical Society.

[1] See Hirsch, Elisabeth Feist, The friendship of Erasmus and Damião de Goes, *Proc. Amer. Philos. Soc.* **95**: 556–568, 1951.

[2] A nephew of Goes told the Inquisition in Lisbon that his uncle had often remarked to him how much he would have liked to die in Flanders. See Henriques, Guilherme, *Ineditos Goesianos* **2**: 21, Lisbon, 1896–1898.

[3] In 1519, Erasmus preferred to go to Basle from Louvain because the university had engaged in anti-Lutheran activities.

[4] Alexander Farnese succeeded to the Papal throne in October, 1534. The exact date of Goes' arrival in Italy is not known. It is certain that he was in Padua in November. In a letter dated November 14, Bembo had written to Erasmus that he had met the Portuguese humanist. Goes published this letter in his collection of letters (for the greater part written to him) which was printed in 1544 in Louvain under the title "Epistolae Sadoleti, Bembi, et aliorum clarissimorum virorum ad Damianum a Goes Equitem Lusitanum." We will refer to this edition as Goes' letters 1544.

[5] A recent most interesting study of this period was published by Jedin, Hubert, *Geschichte des Konzils von Trient* **1**. *Der Kampf um das Konzil*, Freiburg, in Baden, 1949.

[6] The most outstanding example was furnished by Gasparo Contarini, who studied Luther's ideas thoroughly. He was made Cardinal by Paul III and became the leading figure in his cabinet. See Rückert, Hanns, *Die Theologische Entwicklung Gasparo Contarinis*, Bonn, 1926.

[7] See Cione, E., *J. de Valdès, La sua vita e il suo pensiero religioso*, Bari, 1938. Also Bataillon, Marcel, *Erasme et l'Espagne, Recherches sur l'histoire spirituelle du XVIe siècle, passim*, Paris, 1937.

[8] See Bainton, Roland H., *Bernardino Ochino esule e riformatore senese del Cinquecento 1487–1563*, Firenze, 1940.

[9] *Ibid.*

[10] See Benrath, C., Geschichte der Reformation in Venedig, *Schriften des Vereins für Religionsgeschichte*, V. Jahrgang, 18, Halle, 1887. See also Hirsch, Elisabeth Feist, Damião de Goes und die Reformation, *Theologische Ztschr.*, Basel, 42 ff., Januar–Februar, 1950.

[11] See Ehse, St., Kirchliche Reformarbeiten unter Paul III vor dem Trienter Konzil, *Römische Quartalschrift* **4**: 153 ff., 897 ff., 1901.

These Cardinals were to become Goes' friends. All of them, including the Pope, had friendly relations with Erasmus, whom they greatly respected. Paul III hoped to enlist Erasmus' help in his effort to reconcile the Protestants.[12]

Apart from the religious atmosphere, in which open discussions were permitted, Goes must have been pleased with the devotion to Humanism which he found in Padua. One of the most outstanding representatives of Humanism in Italy was the philosopher at the University of Padua, Lazzaro Bonamico.[13] He was the much admired friend of Erasmus, Bembo, Sadoleto, Pole, and other famous men of his time. Damião de Goes had hoped to engage him as a private teacher; but on Erasmus' advice he changed his plan and attended the philosopher's lectures at his house.[14] Their relationship developed quickly beyond the master-and-student contact into a real friendship. What were the deeper reasons for their mutual sympathy?

Bonamico's study of antiquity had led the philosopher to adopt some kind of pagan religion. He was convinced that the study of ancient writers, especially Aristotle, would eventually improve the moral life of his age. One of his friends, Cardinal Pole, was deeply worried about his lack of concern with the Catholic faith.[15] But Bonamico showed considerable interest in the Protestant reform. Like Goes he was in correspondence with Gryneus, one of the outstanding Protestants of Basle.[16] The Portuguese humanist found in the philosopher a man with whom he was able to talk freely about the religious doubts which troubled him. At this stage of his development, Goes needed such an exchange of thoughts more than anything else.

The main purpose of Goes' taking residence in Padua was, of course, to deepen his humanist knowledge under the expert guidance of Bonamico. Though nothing is known about the specific subjects that interested him in Padua, it is safe to assume that Goes followed pretty closely in the footsteps of the philosopher.[17] On the other hand, it can be concluded from their later correspondence that Bonamico supported most enthusiastically Goes' own scholarly endeavors.[18] From Louvain, Goes sent Bonamico the *History of the Wars in India,* which the latter read with great satisfaction.[19] The philosopher commented that the pamphlet not only told an impressive story of the achievements of the Portuguese in India, but gave hope at the same time of the survival of Western culture, which was threatened by barbarous nations.[20] Bonamico wanted to write a history of the wars against the Turks and had

[12] See Pastor, L. *History of the Popes,* ed Kerr, **12**: 542, St. Louis, Mo., 1912.

[13] For Bonamicus' biography see Marangoni, G., Lazzarus Bonamicus e lo studio padovanio, *Nuovo Archivo Veneto,* ser. 3, **1**: 118-151; **2**: 131-196, 1901.

[14] See Hirsch, Elisabeth Feist, The friendship of Erasmus and Damião de Goes, 565.

[15] In an interesting letter to Cardinal Sadoleto, Pole expressed his fear that Bonamico might be drifting away from religion. He asked Sadoleto to use his influence with Bonamico and to impress on him the need for the solution of Christian questions. Pole's letter was written from Padua on October 15, and Sadoleto's answer on December 9, 1534. Both letters are published in *Epistolae Clarorum Virorum selectae.* Coloniae Agrippinae, 1586, pp. 29 ff. and p. 59, respectively.

[16] On Bonamico see Marangoni, *op. cit.* **2**: 164. On the warning that Erasmus had given Goes in regard to this correspondence see Hirsch, Elisabeth Feist, The friendship of Erasmus and Damião de Goes, 564.

[17] In 1538 Goes published a Portuguese translation of Cicero's work on "Old Age" under the title "Livro de Marco Tullio Ciceram, chamadam Catum major ou da velhice." This should be regarded as a direct result of his studies with Bonamico.

[18] Three letters of Bonamico to Goes are extant. The respective dates are: Padua, May 1539; Padua, September 1539; Padua, July 1539. See Goes' letters, 1544.

[19] Bonamico does not mention the exact title of the pamphlet. However, from a letter written by Cardinal Madruzzo to Goes in 1539 we learn that the title was the following: "Commentarii rerum gestarum in India citra Gangem a Lusitanis anno 1538," Louvain, 1539. For Madruzzo's letter, see Goes' letters, 1544. The *Commentarii* were translated into Italian. According to Goes' own words, it was he who made the translation on the advice of Bembo: "Dum agerem Patavii ubi tua humanitate mire me totum devinxisti, inter caetera, quae ex Lusitanica ad me perferebantur accepi de bello Cambaico nonnulla quae (ut petisti) ex Lusitanica lingua in Italicum sermonem converti." (See *Diensis Nobilissimae Carmaniae seu Cambaiae urbis oppugnatio,* 115, ed. Coimbra, 1791.) On the other hand, Marcel Bataillon in his study of Goes entitled Le Cosmopolitisme de Damião de Goes in *Revue de Littérature comparée,* January-March, 1938, quotes on p. 42, n. 3, an Italian translation of the same work made by Giovanni Palus of Venice.

The *Commentarii* must have been soon withdrawn from circulation. They were replaced by a more comprehensive description of the wars in India. The title was changed to "Diensis Nobilissimae Carmaniae seu Cambaiae urbis oppugnatio." The work was dedicated to Bembo and was probably published not later than 1540. See also Joachim de Vasconcellos, Goesiana, *Archeologia Artistica* **7**: xx, 1879, where he remarked that the second edition of the *Commentarii* was newly edited.

[20] See the letter written by Bonamico to Goes from Padua, September, 1539, in Goes' letters 1544.

talked about the plan to Goes.[21] For him history was the realm where the unfolding of the human mind could be observed. Thus he pointed out in one of his letters to Goes: "I have read with pleasure the history which you have recently published since I usually enjoy this kind of literature. I strongly believe that it applies no less to philosophy than to public and private matters." [22] The philosopher and the Portuguese humanist agreed also in their admiration of the progress being made by their age: "You believe that each one of us should take notice that the superiority of our times cannot be explained better than by the art of printing and the discovery of the New World. I have always been of the opinion that those achievements are to be compared not only with ancient times but with immortality itself." [23]

Damião de Goes was a generous friend. Soon after his return to Louvain he sent his teacher in Padua some parchment and a globe. Bonamico thanked him warmly for the gifts, adding a touch of good humor when he reported to Goes that he considered the mutilated arc an ill-deserved misfortune since he had never hurt anybody, not even an innocent bird.[24]

There are indications in his letters that Bonamico's friendship for Goes was quite deep-seated. He had heard about Goes' marriage from mutual friends before the latter had informed him about the happy event. This had somehow hurt his feelings.[25] He waited for confirmation of the wedding by Goes before he sent his congratulations. The letter arrived and Bonamico found warm words to express his joy over Goes' marriage. He also voiced the hope that his friend "may have children who are like him." [26]

Goes must have visited freely in the philosopher's home. He knew Bonamico's father to whom he sent his greetings.[27] In his teacher's house he also got acquainted with a man who played a great part in the Catholic reform movement. This was Cardinal Pole.[28] How far their acquaintance developed is hard to say. At any rate Goes admired the Cardinal greatly. In his letter to him he predicted that Pole would some day be King of England.[29] He was eager to have the Cardinal's publications and asked for a copy of one of his pamphlets which he had seen in manuscript many years before it was actually printed.[30] Goes sent to Pole his book dealing with the faith of the Abyssinians, adding the good advice: "to set his mind to read it that he may understand how the Christian faith (which is deserted in our days among us) is propagated in other places." [31] Pole in his answer noted with deep regret the abuse Goes had to suffer from a common friend, the Englishman, Richard Morrison.[32] If Bonamico was somehow indifferent towards the Catholic faith, Goes had found in Pole a friend who combined an unquestionable love for the Church with a keen mind eager to absorb the new thinking of his times.[33]

[21] See G. Marangoni, *op. cit.* **2**: 151–152.

[22] "Legi perlibenter historiam a te nuper editam, non modo quia delectari soleo hoc genere literarum, et id vehementer existimare ad res et publicas et privatas nec minus ad philosophiam ipsam pertinere." Letter from Padua, September 1539, in Goes' letters 1544.

[23] "Cave enim putes quicque nostra superiorave tempora magis posse illustrare quam imprimendi rationem et novem orbis inventionem quae duo non modo cum antiquitate conferenda sed et cum immortalitate ipsa comparanda semper iudicavi." *Loc. cit.*

[24] Letter from Padua, July, 1539, in Goes' letters 1544.

[25] "Quae antea de matrimonio audieram quae que iucundissima fuerant eadem multo iucundiora extiterunt lectis tuis literis quas heri accepi non enim solum certiora existimabam quae a te afferebantur sed etiam humanitas tua ad me scribens sustulit pene subrusticam verecundiam meam. Nam cum ad te vellem scribere gratulationem quas meas partes esse putabam impediebar tamen eo timore ne viderer scribendo exigere quod mihi non debebas sed liberalitas ut solet tua obtulerat. . . ." Letter of May, 1539, in Goes' letters 1544.

[26] *Ibid.*

[27] *Ibid.* "Patrem meum caeterosque (ut jubes) tuo nomine salutabo."

[28] Pole stayed in the house of Bonamico in 1534. See his letter to Sadoleto quoted above, n. 15.

[29] Goes' letter is published in *Epistolarum Reginaldi Poli S.R.E. Cardinalis et aliorum ad ipsum*, part III Brixiae 1748 ed. Cardinal Angelus Quirinus, p. 37–38. Goes' letter and Pole's answer were reprinted by Marcel Bataillon, Damião de Goes and Reginald Pole, *O. Instituto* **79** (1): 8–11, Coimbra, 1930.

[30] *Ibid.* The treatise was entitled "Pro ecclesiasticae unitatis defensione." A copy of the manuscript had reached Antwerp where the English ambassador burned it. It was published without Pole's knowledge in 1539. The Cardinal agreed to its publication only in 1554.

[31] *Ibid.*, 9. "ad quem legendum te oramus animum inducas ut intelligas quousque Fides Christi (indies iam apud nos deficiens) aliis in locis propagetur."

[32] *Ibid.*, 8 and 10. Richard Morrison had received financial support from both Pole and Goes.

[33] See Jedin, Hubert, *Geschichte des Konzils von Trient*, 296, where he mentions the fact that Pole's doctrine of justification was suspect. Pole had contact with Vittoria Colonna who protected Ochino, the poet Marc-Antonio Flaminio and others who were considered heretics. See Schenk, W., *Reginald Pole, Cardinal of England*, London-New York-Toronto, 1950, esp. ch. 6, "Faith, Works, and Justification."

Padua opened for Goes the door to another friendship with a high-ranking Catholic whose influence on his intellectual development must be counted as strong. Cardinal Bembo was congenial to Goes in many respects. Both men were enthusiastic supporters of the arts. During his residence in Flanders, Goes had acquired many valuable paintings.[34] One of Goes' biographers noted that his house in Lisbon was like a museum.[35] It may be recalled here that Bembo was not only the artistic adviser of Isabel d'Este but also a close friend of Raphael.[36]

Bembo had retired to Padua in 1521. He had met the Portuguese humanist soon after his arrival in Italy, and he reported to Erasmus, who had recommended Goes to him, that he was pleased with his personality.[37] Goes was a sociable person who enjoyed the pleasures of life. So did Bembo. Though a loyal Catholic, the later Cardinal in Paul III's cabinet allowed himself many liberties not only in regard to his personal conduct but also in his thinking about religious questions.[38] Bembo contributed greatly to the liberal atmosphere in Padua which meant so much to Goes.

It was Bembo who gave Goes the most encouragement in his writing. He was fascinated by the reports his friend received in regard to the Portuguese affairs in India. Bembo, who was engaged in writing a history of Venice, had much understanding of the Portuguese adventures in India. Contrary to many of his contemporaries, he did not engage in criticism of the Portuguese exploitations in the Far East, but was much more inclined to see its glory. It was he who urged Goes to translate the messages which he received from Portugal about the wars in India into Italian.[39] Moreover, Bembo insisted that Goes should write a comprehensive story of the events related to the Far East. With good reason Goes dedicated his "Diensis Nobilissimae Carmaniae seu Cambaiae Urbis Oppugnatio" to the Cardinal: "I am not able to find enough thanks to offer you for your kindness. . . . But I am sending you now what I know will be pleasing to you. . . . It did not escape me how much you enjoyed the deeds of our Portuguese and especially those which were done in the interest of our holy faith. . . ." [40] In the letter in which Bembo thanked Goes for the pamphlet he reminded him of his promise to put into print the whole story of what the Portuguese had done in India and will accomplish there in the future.[41]

Goes considered Bembo one of his most trusted friends. Several times he asked the Cardinal for his good services either in his own or a friend's interest. It was Bembo who presented to the Pope Goes' writing which dealt with the faith of the Abyssinians.[42] Furthermore, the Portuguese hu-

[34] About the many gifts of paintings he made to churches in Portugal see Goes' own statement before the Inquisition in: Henriques, *op. cit.* **2**, pp. 88 ff. Also in Bell, Aubrey, F. G., *Um humanista Portugues Damião de Goes,* translated into Portuguese by A. A. Doria, pp. 28 ff., Lisbon, 1942. See n. 102.

[35] Viterbo, Sousa, Estudos sobre Damião de Goes, *Novos Estudos,* ch. 8: 92, Coimbra, 1900.

[36] See Cian, Vittorio, Pietro Bembo, *Giornale Storico della Letteratura Italiana* **88**: 225–255, 1926, esp. p. 244.

[37] See above, n. 4. Also Hirsch, Elisabeth Feist, The friendship of Erasmus and Damião de Goes, 564. Bembo's words in regard to Goes are the following: "posteriore me rogas ut Damiano Lusitano Patavium venienti ad ludum literarum ne desim. Ego vero cum illum libenter vidi est enim quemadmodum scribis atque ut ex duobus ejus congressibus et sermonibus elicui optimis moribus et omni elegantia et suavitate sane praeditus itaque lucrum mihi videor fecisse ex tuis literis qui juvenem mirificum ut cognoscerem effecisti tum me ei largiter obtuli petiique ut meis rebus omnibus meque ipso suo iure uteretur."

[38] See Pastor, *History of the Popes* **8**: 193 ff.

[39] See above, n. 19. In the dedication to the Diensis. . . . Urbis Oppugnatio, Goes mentioned Bembo's enthusiasm for the Portuguese adventures in India. See ed. Coimbra, 1791, pp. 113 ff.

[40] *Ibid.,* 113–114. "Quid tibi pro tua assidua erga me, dignissime Cardinalis humanitate gratiarum referam reperire nequeo. . . . Nunc vero mitto id quod gratum scio tibi fore. . . . Quantum delecteris gestis nostrorum Lusitanorum non me fugit praesertim illis quae pro bono sacrosanctae fidei nostrae geruntur. . . ."

[41] Letter dated Rome, January 1540 ed. Goes' letters 1544. Goes had published two other letters from Bembo to him. One is dated Venice, April, 1539, the other Rome, January, 1541. In addition, two letters written by Goes to Bembo from Louvain in September 1539 and October 1540, respectively, were published by De Vocht, Henry, Damião de Goes and his Oratio postliminio ad Universitatem, *Monumenta Humanistica Lovaniensia, texts and studies about Louvain humanists of the first half of the 16th century,* pp. 694–696, Louvain, 1534.

[42] See Bembo to Goes, January, 1541, "Sed ad librum revertor cuius ego exemplum una cum tuis literis Pontifici Maximo sicuti mandas reddidi quod ille fuit plane gratum." The title of the book was *Fides, Religio, Moresque Aethiopum.* It was dedicated to Paul III. In his letter to Bembo written from Louvain in 1540 Goes had explained to Bembo why he had dedicated the book to the Pope. He did not expect any material advantages from Paul III but he desired to show the Pope his devotion to the Christian religion: "Sed ut illi me devotum et incrementis illius Maiestatis et Christianae fidei ex animo studere declarerem." The book was put on the index in Portugal immediately after publication. See Henriques, *op. cit.* **2**: 47.

manist appealed to Bembo for help when he found himself in French captivity.[43] He had written several letters to the Cardinal mentioning his ill-fortune, but no reply or encouragement had come from Bembo.[44] Soon after his return to Lisbon, Goes had written again to the Cardinal informing him of his return to his native country.[45] In his answer Bembo expressed his regret "that I was unable to intervene because I was separated by space far from you. I also was uncertain who it was who held you captive." [46]

After Goes had taken residence in Louvain again, his friend Peter Nannius complained that the reading of the hour book interfered with his many scholarly duties. The Portuguese humanist appealed to Bembo to intervene with the Pope that he might grant Nannius dispensation from his religious obligation. The Cardinal complied with Goes' wishes without, however, meeting with success.[47]

In his letters Bembo displayed sincere interest in Goes' personal life. He congratulated him warmly on his marriage in the same note in which he spoke with great modesty about his election to the cabinet of the Pope.[48] Goes, on the other hand, did not miss an opportunity to inform Bembo about every happy event in his life. He wrote to the Cardinal about his wife's pregnancy; Bembo replied "that she may bring forth a preserver and a little son who is like his father." [49] When Bembo finally heard that a son had been born to Goes, he shared the joys of a father with his friend: "I am happy and congratulate you on your little son. Though I have never seen him, only because he is yours I think he must be lovable. I love him very much and ask you to give him a kiss in my place." [50]

Damião de Goes was not the first "cultural ambassador" from Portugal whom Bembo had met. On his travels through Europe, the Portuguese poet André de Rèsende became acquainted with Bembo.[51] Since that time they had exchanged letters, but the poet was somehow a reluctant correspondent. In one of his letters to Goes Bembo remarked that he would be very glad to receive news from Rèsende.[52] It seems that Goes was able to revive the poet's interest in Bembo after he had come back to Portugal. In his letter of 1546 the Cardinal mentioned the fact "that he had received with the greatest pleasure the hexameters by your famous poet Rèsende which have reached me together with your letters." [53]

There can hardly be any doubt that Goes encouraged another compatriot and friend of his to write to Bembo. This was Jorghe Coelho, son of Nicholas Coelho, who went with Vasco da Gama to India. He had ambitions as a poet and served as secretary to Cardinal Henrique, who was to introduce the Inquisition into Portugal.[54] Goes was in constant contact with Coelho, who provided one channel through which he kept informed of events in his homeland.[55] Bembo wrote to Coelho for the first time in 1541.[56] In the above mentioned letter which Bembo sent to the Portuguese humanist in 1546 he noted the fact that Cardinal

[43] In 1542 Louvain was invaded by the French. On that occasion Goes who went as negotiator to the enemy camp was made a prisoner. The episode was studied with great care by De Vocht, Henry, *loc. cit.*, 619–698.

[44] We know this only from a letter written by Bembo to Goes from Rome, October 1546. See Petri Bembi Card. *Epistolarum Familiarum* **6**: 395–396, Venice, 1552.

[45] *Ibid.*, 395.

[46] *Ibid.*, 395, "quibus quidem mederi non poteram tam longo abs te locorum divisus spatio: tam incertus qui essent qui te captivum fecerant."

[47] See Bembo's letter of January 1540 and Goes' reply of October 1540 in Goes' letters 1544 and De Vouht, *loc. cit.* resp. Nannius was the successor of Goclenius as teacher of Latin in Busleiden College at Louvain. See Polet, Amédée, *Une gloire de l'humanisme belge, Petrus Nannius 1500–1557*, 25, Louvain, 1536.

[48] See Letter from Venice, April, 1539 in Goes' letters, 1544.

[49] "Ei dices ut se tibi sospitem servet et filiolum patri similem edat cum pariendi tempus venerit." Letter of Rome, January, 1540.

[50] "gaudeo et gratulor atque illum quem nunque vidi quia tamen tuus est et amabilem puto esse et vehementer amo ei tu suavium des velim." Letter of 1541.

[51] For biographical details on the life of Rèsende see Braankamp, Freire, *Noticias da Vida de André de Rèsende*, Ed. Arquivo Historico Português, 1916.

[52] Letter of April 1539. The same is true in regard to Erasmus, see Hirsch, Elisabeth Feist, The friendship of Erasmus and Damião de Goes, 556, n. 1.

[53] "Resendii tui poetae illustris hexametri quos accepi una cum tuis litteris maiorem in modum me delectaverunt." Letter to Goes 1546, p. 395–396.

[54] For a short biography of Coelho see the ed. of Erasmus' letters by Allen-Garrod, **11**, n. to ep. 3043, line 12.

[55] Goes published two of Coelho's letters to him in 1544. One was a long letter written in 1540, the other shorter dated 1541. Both letters refer to a work of Coelho, *De Patientia Christiana*, of which he wanted a judgment from Goes. From the second letter it is obvious that Coelho had sent his poem also to Bembo and Sadoleto who had both replied to him.

[56] See ed. of Bembo's letters, pp. 381–382. Bembo remarked in his letter to Coelho that he counted him among his friends in spite of the fact that he had not heard from him before.

Sadoleto had read Goes' letters and "sends his greetings and congratulations." [57]

With the name of Sadoleto another chapter of the many important friendships which Goes made in Italy opens up. While in the case of both Pole and Bembo the give-and-take was pretty equally divided, Goes met in Sadoleto a man to whom his diplomatic experiences were of the greatest value. Moreover, their relationship received its impulse not so much from personal contact and the resulting warm personal feelings, but from a similar dedication to a practical task before them. Sadoleto said in one of his letters to Goes that they "never developed any familiarity in each other's presence." [58]

Sadoleto became Cardinal soon after Paul III was elected Pope. Together with his friends Pole and Contarini he was a leading figure in the movement for reconciliation between the Church and the Protestants.[59] Sadoleto had cast an eye especially on Melanchthon, whom he hoped to bring back to the Catholic fold.[60] At a time when he tried to establish some contact with Wittenberg he received the visit of a certain Petrus Bechimius. Peter was not only a friend of Bembo and Erasmus; he was also a companion of Damião de Goes in his travels through Germany.[61] Thus the Portuguese humanist was well known to him. In his conversations with Cardinal Sadoleto, Bechimius recommended Damião de Goes as the connecting link between the Cardinal and Wittenberg. "Peter the Bohemian, a man of nobility and learning, who is in character as in many other respects superior, is present here and has spent several hours with me. He told me a lot about you." [62] In the letter which Sadoleto sent to Goes through Bechimius he was careful not to go into any detail about the mission he was to entrust to the Portuguese humanist. He left all explanation to be given orally by Bechimius. It is interesting to note that Goes is much less cautious in his immediate answer and acceptance of his role as intermediary.[63] He was more than frank in his statement that he "contracted no mean friendship" with the Protestants whilst on a diplomatic mission for his king in Germany.[64] Sadoleto had asked Goes also to forward a letter to Melanchthon which he had sent with Bechimius. Goes was able to inform the Cardinal that the letter was already on its way to Wittenberg. He had joined to it Sadoleto's speech in regard to the planned Council of Mantua in order to give Melanchthon an idea of his "devotion and love for the Church." [65] He attached to his note to Sadoleto a document which he had recently received from the Protestants; in it they expressed their intention not to attend the Council of Mantua. Bechimius had told him that the Cardinal had not yet seen it.[66] Goes could hardly give any stronger proof of his continual contact with his friends among the Protestants.

Sadoleto kept the Portuguese humanist informed about the progress of their affairs. In January 1538 he wrote to Goes about the many

[57] "Sadoletus Cardinalis legit tuas ad me letteras; tibique gratulatur et multam salutem dicit." P. 396.

[58] "etsi nulla unquam inter nos praesentes intercessit familiaritas. . . ." Letter of January 1540 in Goes' letters 1544. Goes published four letters of Sadoleto written to him. One is dated Rome, July 1537, the second, January 1538, the third, January 1540, the fourth, September 1540. In addition Goes had published the letter he wrote to Sadoleto, July 1537, as answer to the message sent through Bechimius.

[59] On Sadoleto see Ehse, *Kirchliche Reformarbeiten* and Pastor, *History of the Popes.* It is hard to believe that Goes should not have met Contarini. But he is never mentioned in the correspondence with the other Cardinals nor are any letters known that they may have exchanged.

[60] See the excellent study by Gustav Kawerau, "Die Versuche, Melanchthon zur katholischen Kirche zurückzuführen, *Verein für Reformationsgeschichte* **19**, Halle, 1902.

[61] Allen-Garrod, *op. cit.*, publish a letter from Bechimius to Erasmus (ep. 3027). The editors remark that very little is known about Bechimius. Before the Inquisition Goes mentioned that Bechimius "foi seu companheiro no estudo que elle confessante andara por toda a Alemanha e que estivera em Wittenberg onde fallara com Martin Luthero e Felipe Melanchthon: e por isso lhe mandava esta carta. . . ." Henriques, *op. cit.* **2**: 35. Also quoted by Carvalho Reys, *Damião de Goes, Opusculos Historicos,* 40, Porto, 1945.

[62] "Petrus Bohemus praestanti vir et nobilitate et doctrina idemque et morum et partium optimarum cum aliquot horas fuisset mecum multa de te mihi narravit." Letter of Rome, July 15, 1537.

[63] Goes' letter to Sadoleto was dated from Padua July, 1537.

[64] *Ibid.*

[65] Sadoleto's letter to Melanchthon caused something like a sensation in Protestant circles. Sadoleto had praised Melanchthon's devotion to Humanism and the latter was impressed with it. But since Melanchthon did not trust the Cardinal, his reaction was delayed. Luther was furious. See Gustav Kawerau, *Die Versuche, Melanchthon zur katholischen Kirche zurückzuführen,* 34 ff. Sadoleto's letter is printed in *Corpus Reformatorum* **3** (1587).

[66] In a note dated February 24, 1537, the Protestants had refused to attend the Council. This note must have been known to Sadoleto in the spring at the latest. Had Goes received a comment by Melanchthon on the planned Council of Mantua which he sent on to Sadoleto?

troubles that resulted for him from the letter he had sent through him to "the German people's teacher": "You should know how much anxiety has come to me from the letter which was sent to your friend. It was spread among the Germans and has aroused no small infamy and suspicion against me. As if I had deserted the former cause and wanted to do a job with the Lutherans from which I shrink back by a far different inclination." [67] Nevertheless, Sadoleto was convinced of the righteousness of his cause and he was not willing to give up working for peace among the fighting religions. He asked Goes to support him further in this work and to give him his advice in his letters.[68]

How long did the cooperation between the two men last? Goes had written to Sadoleto from Louvain and had also sent him his writings.[69] From the Cardinal's answer, which is dated January, 1540, we learn that Goes had advised him to discontinue his correspondence with the Protestants: "You admonish me like a friend to write no more to those people whom you have known and I am grateful to you. I will be glad to comply with the very good advice you give me." [70] Moreover, Sadoleto explained to Goes that he had written to Melanchthon and Sturm not on his own volition but on the suggestion of the "Cardinal from Paris." However, the Cardinal did not hesitate to add that he had followed his conscience in corresponding with the Protestants: "If it is true that this correspondence, as you write, does not bring me honor, it has, nevertheless, satisfied my conscience, since in matters pertaining to God and religion one should look at more than one's own honor." [71]

When and for what reasons did Goes change his mind in regard to the contacts with the Protestants? On his arrival in Padua Goes enjoyed, as was noted before, the liberal atmosphere in the city.[72] He even dared to receive the visit of an old acquaintance, Roque d'Almeida, who had come from Wittenberg with letters from Luther and Melanchthon.[73] Roque, who had changed his name to Jeronimus of Pavia, was invited by Goes to stay with him because he was without any means. Goes confessed before the Inquisition that he sent him away after his talk had become too compromising.[74] At least until early in the year 1538 Goes still openly admitted his contact with the Protestants. In fact, the Portuguese humanist was rather careless in that respect. His later accuser before the Inquisition in Portugal seemed to have been present at many compromising conversations. Simão Rodrigues, who became a prominent Jesuit, denounced his countryman soon after Goes' return to Portugal in 1545. He referred to discussions about faith between Goes and Roque where he heard "that Damião de Goes said human beings can be certain that they have grace." [75] Moreover, Goes had told Rodrigues that Sadoleto had made use of his good connections with the Protestants.[76]

Goes' rather sudden change of attitude toward the Protestants, although it cannot be questioned, has yet to be explained. It is important in this connection to point out that Goes, admitting before the Inquisition that he once held some religious errors, told his interrogators at the same time that he confessed those errors when he was in Padua.[77] It is known that Goes returned to Louvain late in 1538. There are strong reasons to believe that he broke his ties with his Protestant friends some time during the year 1538.

[67] "Etenim scito magnas mihi sollicitudines accessisse ex ea epistola quae ad amicum tuum missa est illa enim inter Germanos vulgata non parum in me infamiae et suspicionis concitavit quasi ego pristina causa desciscere et cum Luteranis commercium habere velim a quo ego sensu longissime abhorreo."

[68] *Ibid.* "Cactera quae ad causam huiusmodi pertinebunt literis inter nos ut occasio affuerit agentur, atque explicabuntur, confido enim te cui et in virtute et in doctrina et in amore erga me permultum tribuo assiduum mihi et benivolum adiutorem affuturum."

[69] This can be learned from Sadoleto's letter dated January 1540.

[70] *Ibid.* "Quod me admones ne quid ad illos homines quos nosti amplius scribam, amice facis, habeoque gratiam tibi optimi consilii quod mihi das cui libenter equidem obtemperabo."

[71] *Ibid.* "Sed etsi ea res, ut scribis, honori mihi non est tamen quod in causis Dei et religionis magis spectare debemus conscientiae a me meae satis est factum."

[72] See above.

[73] For his relations with Roque see Hirsch, Elisabeth Feist, *Damião de Goes und die Reformation*, 49 f.

[74] See Henriques, **2**: 50. He also told the Inquisition that he had destroyed Luther's letter immediately. *Ibid.*

[75] "Damião de Goes dizia que os homens podião ser certos que estavam em graça." See Henriques, **2**: 13.

[76] In his first denunciation of Goes before the Inquisition in Evora in 1545 Rodrigues referred to his role as intermediary between Sadoleto and the Lutherans with the following words: ". . . Goes tinha muita auctoridade entre os lutheranos e que o ditto Damião de Goes dixera a elle testemunha que hum Cardeal cujo nome ao presente lhe nao lembra lhe escrevera a elle Damião de Goes como a pessoa que com os lutheranos poderia acabar alguma cousa escrevendo-lhe pera que nisse entendisse." Henriques, **2**: 6.

[77] Henriques, **2**: 58.

At that time another important event in Goes' life took place. It seems that his decision to leave Italy must have been connected with his plan to get married. Unfortunately, the exact date when the wedding took place is not mentioned in any available source. However, in the "Nobilario" (in all likelihood written by Goes himself but extant only in manuscript copies) it is said that the marriage took place in 1538.[78] Goes took as his wife Johanna van Hargen, sister of his good friend Splinter, with whom he shared the apartment in Padua.[79] Splinter was known to him from Flanders and Goes probably had met his future wife there.[80] It is safe to assume that the Van Hargens were devout Catholics, the more so since it is known that Splinter was a student and friend of Christopher Madruzzo, who played an important part in the Council of Trent.[81] Did the Dutchman insist that his Portuguese friend cast off all shadows of his Protestant leanings as price for the hand of his sister?

Splinter probably had introduced Goes to the Cardinal of Trent. Among all his friends who belonged to the high ranks of the Catholic hierarchy none may have been as congenial to him as Christopher Madruzzo, of whom Pastor said: "Madruzzo, whose portrait Titian painted, . . . was a man of very worldly temperament and a great friend of artists and men of learning. . . . Glarean dedicated his book on music to him."[82] Hubert Jedin, on the other hand, pointed out that none of his manifold interests went as deep as his love of music. He also stressed the fact that, though Madruzzo tried to work for reconciliation between Catholics and Protestants, he was far less talented in that respect than either Contarini or Morone; Jedin believed in addition that he was less of a scholar than Sadoleto or Bembo and much more for outward pomp than any of Goes' other friends in Italy.[83] Their common love of music may be the cause of the sympathy which developed very fast between them. It is also true that Madruzzo himself had the impression that their personalities were very similar: "I do not believe that our friendship is built on such thin foundation that such a little matter as the discontinuance of letters for the time being can make it totter. This cannot be since our friendship took its origin not through the enticement of Fate but through the likeness of our souls and customs."[84] Madruzzo found in the same letter still stronger words to express the amazing attraction that he experienced towards Goes: "Yes, indeed, a certain secret force determined that I should love you."[85]

Madruzzo, who outlived Sadoleto and Bembo, both dead in 1547, received one of Goes' later publications, of which he was quite proud. This was the "Urbis Olissiponis descriptio" printed in 1555. In the accompanying letter Goes pointed out that he sent him his writing not only because he knew that the Cardinal was interested in his work but "that he sees of what his Damião is able who in whatever circumstances he may find himself is yours and will be yours. This was so at the

[78] This writer has a photocopy of the "Nobilario" which belongs to the National Library in Madrid. See MS. 1449 f. 140 v., where it says: "with permission of the same Sire (John III) he married in the year 1538 in the county of Holland in the town of the Hague. . . ." (English translation by the writer from the original Portuguese.)

[79] See Hirsch, Elisabeth Feist, *The friendship of Erasmus and Damião de Goes*, 565, n. 92.

[80] Though the Van Hargen family originated in Holland, Splinter and Johanna must have spent many years in Flanders. After Goes returned to Portugal, Splinter stayed on in Louvain and Goes later sent to him his two oldest sons, who were to prepare for a religious career. This fact is mentioned by Goes in a letter written to Madruzzo from Lisbon in March, 1555. The letter is published here for the first time. See Appendix 2. I owe the meager information about the Van Hargen family which exists in the archives in Holland to the courtesy of Mr. H. Hardenberg, archivist of the Algemeen Rijksaechief's Gravenhage, Holland. Also, Professor M. de Jong, The Hague, was kind enough to offer some information. According to those sources Johanna and Splinter had another sister, Maria. The parents had died before the children reached maturity. Later Splinter acted as head of the family. He became dike-reeve of Schieland and paymaster general of the abbey of Rijnsburg. Though the wedding date is not mentioned in the documents, the Archives of the Nassau Domain Council (inventory Hingman no. 6524 f. 18v–19) mention the fact that "Damião de Goes, husband of Johanna van Hargen, received on 10th April, 1539, by decision of the Loan Court of Naaldwijk as a loan the estate 'De Lage Woerd' at Naalwijk as a result of an order given by his brother-in-law, Splinter van Hargen."

[81] In his letter to Madruzzo from Lisbon Goes calls Splinter "alumnum tuum." See Appendix 2.

[82] See Pastor, *History of the Popes* **13**: 203, n. 1. Titian's painting is in New York.

[83] Jedin, Hubert, *Geschichte des Konzils of Trient*, 452 ff.

[84] "Nec credo amicitiam nostram tam labilem fixisse pedem ut ex re tam minutula intermissione scilicet literarum ad tempus labefactari queat maxime quum originem sumpserit non ex Fortunae blanditiis sed ex morum et animi similitudine." Letter of May 1541, in Goes' letters 1544. Goes published in 1544 another letter of Madruzzo dated November 1539.

[85] "Imo vi quadam occulta traxit ut te amarem. . . ."

beginning of our friendship." [86] In the same letter Goes informed Madruzzo that he had now six sons and one daughter "whom he considered his treasures beyond any riches." [87]

In 1539 Madruzzo became archbishop and Goes congratulated him on this occasion.[88] He had also asked the archbishop for support in a matter which is not mentioned: "If I can do anything in your interest in case I should be solicited you know that nothing is closer to me than to please you in everything." [89]

Splinter van Hargen wrote a long letter to Madruzzo from The Hague in March, 1543, explaining to the archbishop that after a long illness had prevented him from joining him in Trent he had to postpone his trip to Italy again because of his brother-in-law's ill fortune.[90] After Goes was imprisoned by the French his wife had left Louvain and joined her brother in Holland. At this writing, however, Splinter was able to report that Goes was free after successful intervention by the Portuguese king. Several months later Goes wrote himself to Madruzzo assuring him that his affairs looked much better "than at the time when I wrote to you." [91]

The fact of Goes' imprisonment had caused considerable anxiety among his friends in Italy. It was mentioned also in a letter to the Portuguese humanist by John Magnus, former archbishop of Upsala, whom Paul III had called to Italy for participation in the planned Council of Mantua.[92]

John Magnus awaited the opening of the Council in Vicentia where he met Goes again. In 1529 Goes had first seen the exiled archbishop in Danzig, where they had had stimulating talks.[93] How different must have been their personal contact this time! Paul III had promised to lend the archbishop financial support while he was in Italy, but he did not hold fast to his promise. The once famous man had to beg his friends for support.[94] We hardly need the confirming words of the archbishop himself to be certain that Goes did not desert him in his distress: "Take care, most humane Damião, that nothing will make you think that I can ever forget your great friendliness and kindness towards me. I will try to surpass or at least equal them by pious studies." [95] John Magnus also begged Bembo for intervention with the Pope. Since he knew of the Cardinal's friendship with the Portuguese humanist, he sent him the copy of the "librum de moribus Indiae . . . ," which he had just received from Goes.[96] The former archbishop of Upsala was also in correspondence with Splinter van Hargen. In his note to Goes he expressed the hope that some "fresh Northwind may bring him his letter." [97]

Goes made many more friends in Italy than could be mentioned here. He won, for example, the respect and friendship of the later son-in-law of Melanchthon, George Sabinus, who celebrated Goes in a long poem.[98] However, one other contact made by Goes in Italy is rather significant. Before the Inquisition in Portugal Goes related the story that Ignaz of Loyola visited him in Padua in order to apologize for Simão Rodrigues' unfriendly remarks about him: "The said Mr. Ignaz came from Venice to Padua in order to apologize. On which occasion he stayed in my house with some brothers of his order." [99] Goes' meeting with the Founder of the Jesuit order is interesting for several reasons. Though the date of the visit is not established, it is known that Ignaz and his

[86] See Appendix 2.

[87] *Ibid.*

[88] This is obvious from Madruzzo's letter to Goes of November, 1539, where he says: "Caeterum quod mihi de adepta Episcopali dignitate tam amice congratularis meque officii mei admones facis tu quidem candide et sincere."

[89] "Quod si aliquid potero in rem tuam ubi interpellatus fuero senties me nihil antiquius ducere quam tibi in omnibus complacere." *Ibid.*

[90] See Appendix 3 for the publication of the letter. See also n. 43.

[91] See Appendix 1 for first publication of the letter. The letter which Goes mentions here is not extant.

[92] Letter written to Goes from Rome in May 1543 one year before the death of the archbishop. See Goes' letters 44 where he has published another letter written by John Magnus to him dated Rome, April 1541.

[93] See Hirsch, Elisabeth Feist, *Damião de Goes und die Reformation*, 45.

[94] His tragic last years were described by his loyal brother Olaf who followed him into exile and was with him also in Italy. See *Historia Metropolitanae ecclesiae upsalen. . . .* A Joanne Magno Gotho . . . *Opera Olai Magni Gothi eius fratri in lucem edita*, 130 ff., Romae, 1560.

[95] "Cave igitur mi humanissime Damiane ne unquam putes me tantam humanitatem et beneficientiam tuam oblivioni traditurum sed ut ipsam aliquando meis piis studiis superare vel aequare possim conaturum." Letter of 1543.

[96] The letter was published by his brother Olaf in *Historia Metropolitanae ecclesiae upsalen*, p. 153–154. See Appendix 4. The writing of Goes mentioned here is the *Fides, Religio Moresq. Aethiopium.*

[97] "clarissimus vir D. Splinterus cuius literae si quaedoquidem de movis Aquilonaribus ad dominum meum allatae fuerint, gratissime erunt." Letter written 1543.

[98] The poem is published by Goes as Appendix to the edition of Goes' letters of 1544.

[99] "eo dito Mestre Ignacio veio de Veneza ad Padua a se desculpar de min onde pousou em minha casa com alguns irmaes da sua regra." See Henriques, **2**: 70.

companions stayed in Venice in 1537. They could easily have reached Padua from there.[100] Damião de Goes thus received in his house the man who was to become the most outstanding figure of the Counter Reformation at the very same time when his correspondence with Philip Melanchthon was in full swing! A few years later the situation was to change completely, but in 1537 it was still possible—at least in Italy—to call oneself a friend of both Protestant and Catholic leaders. It is quite likely, moreover, that Ignaz decided to make the trip to Padua after he had talked to Cardinal Pole, who may have come to the defense of the Portuguese humanist.[101]

When Damião de Goes left Italy in 1538 with every intention of settling for good in Louvain, he had four important years of intensive study behind him. What were the tangible results of those years? The humanist education which he had hoped to accomplish in Freiburg Goes finally finished as a student of Bonamico. The literary output which followed his residence in Italy gave ample proof that Damião de Goes had become a scholar in his own right.[102] As a person, the Portuguese humanist had reached his maturity. It seemed that the period of "storm and stress" was over. He left Italy as a devout Catholic. Though the fact of his marriage may have influenced his religious change, it was also a sign that he had somehow calmed down. The example of the "Reform" Cardinals had probably helped Goes in his decision to confess his errors. He had met in Bembo, Pole, and Sadoleto, pious Catholics and yet men of liberal thinking. His friends in Italy deserve highest credit for having convinced him that it was possible to be both "modern" and a Catholic. A liberal thinker Goes remained for the rest of his life. It was his ill-fortune to have lived long enough to witness the utter defeat of liberal Catholicism all over Europe. Thus fate finally caught up with him. As a man of seventy he had to pay for the fact that a world had changed and that his past could not be undone, a past which, it is true, he cherished in his memory until his death. In 1572, the Inquisition in Portugal found him guilty of heresy. The accusation was based to a large extent on his connections with Protestants during his years abroad about thirty years before. Portugal's most famous humanist spent the last twenty months of his life in the monastery of Batalha as a prisoner of the Inquisition.

[100] See Bartoli, Daniel, S. J., *Saint Ignace de Loyola, sa vie et son institut*, trad. par Jacques Terrieu, 340, Paris, 1893.

[101] Pole met Ignaz in Venice and was quite impressed by him. He and his good friend Contarini gave him letters of recommendation to the Pope. They thus shared with others the responsibility for the recognition of the Jesuit order by Paul III. See P. Pietro Tacchi Venturi, *Stòria della Compagnia di Gesu in Italia* **2**: 91: "la Dalla Nascita del Fondatore alla Solenne Approvazione dell ordine (1491–1540)."

[102] During his residence in Louvain from 1538 to 1544 Goes published many of his important writings. See the bibliography contained in A. A. Doria, *Um humanista Portugues Damião de Goes*, Lisbon, 1942. Doria's book is a translation into Portuguese of Bell, Aubrey, Damião de Goes, a Portuguese humanist, *Hispanic Rev.* **9**, 1941.

The following three documents were made available to me by courtesy of the Archives of the State of Trent, Trent, Italy. I received microfilms of the three letters and also typewritten copies of the original manuscripts. My own reading of the letters did not differ from the one offered by the archivist.

APPENDIX 1

Corrispondenza Madruzziana, A. 1543, ff. 45–46

Serenissime Princeps.

Reversus ex Gallia, cum jam essem, tibi amplissime Patrone scripsi, literasque per manus Joannis Jacobi Fuggeri transmisi, modo quid scribam, cum Splinterus ad te proficiscatur qui orethenus tibi res meas explicabit, nihil aliud erit nisi, quod scire te velem, negotia mea felicius, nunc, quam e tempore quo ad te scripsi succedere. Nihilo minus, bona mea, ut fama, quam pluris faciebam minime jam laborem, ad octo millia aureorum coronatorum mulctantur.

Reliqum erit, id quod non ignoras, ut tibi denuo in mentem redigam, me totum esse tuum, quo experientia, si opus erit disces.

Vale inclite princeps et nos ama. Lovanij, 5 julij anno 1543.

Amplitudini tuae deditissimus

Damianus A. Goes.

Verso:
Amplissimo domino Christophoro
Madruchio Episcopo
Tridentino, et Brixinensi,
patrono suo Tridenti.

APPENDIX 2

Corrispondenza Madruzziana, A. 1555, ff. 296–297

Serenissime Princeps.

Cum certo sciam, tuae amplitudini gratum fore, quidquid honesti laboris, a me profectum fuerit, hanc meam membratiunculam, qua situm et figuram urbis Olisiponis evulgare volui, tibi mittendam esse duxi. Nec id ea sola de causa feci, sed etiam ut intelleges, quid tuus Damianus valeat, qui in quocunque statu

egerit, tibi is semper erit, qui ab inicio nostrae necessitudinis fuit.

Caeterum Deus nobis praeter, unam feminam, sex masculas proles dignatus est dare, quem thesaurum ego super omnes Coeresi (sic) gazas extollo, quarum duas masculas maiores natu anno superiore hinc ad sororium meum Splinterum ab Hargen alumnum tuum, misi, ut Lovanij sacris literis incumbant. Vale et rescribe quippe quod certum est, nulla dignitas, quam vix maxima, impedire possit, quin verus amicus, amico quamvix tenui non respondeat. Olisiponae secunda die marcij anno 1555.

Tuae amplitudini deditissimus

Damianus Goes.

Verso:
Viro amplissimo domino Christophoro
Madruchio, cardinali tridentino
Tridenti.

APPENDIX 3

Corrispondenza Madruzziana, A. 1543, ff. 77–78.

Reverendissime ac illustrissime Princeps. Ea fuit hactenus fortunae meae iniquitas, variorumque casuum acerbitas, ut huc redeuntem me eadem illa quartana, quae isthinc invitum abegerat, totum a reditu annum excruciarit, nihil aut minimum, vel itineris labore vel patrij soli aere, vel ulla medicorum ope diminuta, ferendo tandem invictum morbum vici, cum iam corporis vires, ingenijque si quae reliquae essent, penitus absumpsisset, valetudine recuperata firmior, omnibus iam ad iter compositis, alio subito infortunio ab incepto revocor. Nam rex Galliae, solito, quo bello inire solet, astu, foederum oblitus, ducis Juliacensis opera conscriptum clam exercitum de repente in Brabantiam immisit duce Martino Rosio geldro nobile, is agros ferro ignique depopulatus, magna abacta preda, etiam urbibus nil hostile cogitantibus terrori esse cepit. Inter ceteras Lovanium adortus, illam sibi dedi poposcit, Lovanienses rei novitate moti, cum nec militum presidia intus haberent, nec facile urbem amplam magis quam munitam soli cives tueri possent, nec aliunde auxilia expectarent, ultro citroque fide data ad colloquium Martini Pretorem et Damianum emittunt ut pecunia ab illo paciscantur, ne exercitum urbi inferat. Dum mandata utrumque perferuntur, Rossiani milites sensim muro succedunt, hunc occupaturi, inde pactis conveniant, cives fraude animadversa tormentis bellicis illos rapellunt. Hanc repulsam aegre ferens Martinus Damianum cum Pretore secum captivos in Galliam abduxit. Habes reverendissime atque illustrissime domine calamitatis nostre seriem que mihi hactenus impedimento fuit quo minus ad obsequia reverendissimae dominationis vestrae in tempore redierim; cum solus essem qui in hac infelicitate sororem solarer, cui unicum ad me perfugium supererat, quam ita afflictam destituere impium mihi esse credebam, nihil hesitans quin excusatio hec cessationis meae dominationi vestrae reverendissimae nimis iusta ac legitima videretur.

Caeterum regis Lusitaniae intercessione, Damianum libertati restituit rex Galliae, summa adversariorum contentione, qui iniquissimum illi redemptionis precium, nempe novem millium ducatorum, constituerant, obtinuit tamen rex Lusitanie ut si causa cognita iure belli captus esse dicatur, mille ducatos solvat, sin minus, in integrum restituatur: quamobrem propediem illum adfuturum spero: Ego vero deprecata prius diutinae morae venia, rebus omissis omnibus, sub finem proximi mensis, ad reverendissimam dominationem vestram recurram, ut diligentia, fide ac offitiorum sedulitate, prioris temporis iacturam compensem.

Reverendissimam ac illustrissimam dominationem vestram Deus in prosperitate diu servet incolumen. In Haga comitis Hollandiae, 3 martij 1543.

Reverendissime ac illustrissime
dominationis vestrae humillimus servitor

Splinterus ab Hargen.

Verso:
Reverendissimo ac illustrissimo
Principi Episcopo Tridentino.

APPENDIX 4

Letter of John Magnus to Cardinal Bembo.

Excerpt concerning Damião de Goes, in Historia Metropolitanae ecclesiae upsalen in Regnis Suetiae et Gothiae a Joanne Magno Gotho Sedis Apostolicae legato . . . opera Olai Magni Gothi eius fratri in lucem edita Romae 1560, p. 153/54.

"Allatus est praeterea quasi eodem tempore quidam liber Louvanii impressus clarissimi viri Damiani a Goes equitis Lusitani de moribus Indorum Sanctissimo Pontifici Paulo III inscriptus, in cuius fine fidele testimonium per modum appendicis erat annexum, quomodo propter fidem Christi tuendam ipse Archiepiscopus a patria trusus erat, in exilium ad urbem Prussiae Gedanum (ubi praedictus Damianus eidem Archiepiscopo sepius de rebus clarissimis alias est locutus) et demum in Italia in civitate Vicentina Anno 1538 ubi generale concilium credebatur celebrandum hunc librum Archiepiscopus misit D. Cardinali S. Crucis cum hac exigua supplicatione annexa. Mitto Reverendissime D. V. librum de moribus Indie sanctissimo D. N. inscriptum quem alias promisi et quia in eius calce non nihil de honestis et piis laboribus meis annexum existat. . . ."

STUDIES IN THE PROBLEM OF DATING THE DEAD SEA SCROLLS*

JOHN C. TREVER

Executive Director, Department of the English Bible, National Council of the Churches of Christ in the U. S. A.

(*Read April 25, 1952*)

THE recent articles appearing in popular American magazines, describing the application of the radioactive "Carbon 14" process of age determination to the cloth found in the Dead Sea cave from which the famed Dead Sea Scrolls came, have fanned the flames of popular interest once again in these ancient documents. While the popular interest and enthusiam have waxed and waned, scholarly interest has continued unabated, as the analytical and critical literature on the subject has reached almost staggering proportions.

For the sake of those who may not be familiar with the subject, it would be well to review a little of the background of the discovery of the Dead Sea Scrolls. High up on the cliffs overlooking the northwest edge of the Dead Sea near 'Ain Fashkha, about seven and one-half miles south of Jericho in Palestine, some Bedouins happened upon a hole on the side of a rock projection early in the spring of 1947.[1] On entering, they found a large cave penetrating some twenty-five feet into the cliff, and about six and one-half feet wide by eight to nine feet high (fig. 1).

On the floor of the cave, the Bedouins found some forty jars, each with a specially prepared cover. Apparently they smashed many of those not already broken. Only two have been recovered intact. With patient labor some have been restored (fig. 2), and archeologists are able to determine the approximate date of their manufacture. There seems to be little question about the fact that the jars were manufactured in the first century B.C., or early first century A.D., during the late Hellenistic or early Roman period. Here, then, is one important means of determining the approximate age of the scrolls found in the cave, since the homogeneity of all the pottery (with the exception of only a few pieces) gives a strong indication that the jars were made for the purpose of storing manuscripts.

Also in the cave the Bedouins found a great deal of linen cloth, much of which was recovered by the excavators in February, 1949. Analysis by textile experts revealed it to be native Palestinian cloth.[2] The radioactive "Carbon 14" test, developed by Dr. Willard F. Libby of the University of Chicago, resulted in a mean date of A.D. 33 for some of this cloth, but a two hundred year plus or minus margin of error must be allowed.[3]

Hundreds of fragments of manuscripts, trampled into the dust of the cave, were picked up by the archeologists. During a recent leave of absence for the study of the Dead Sea Scrolls the writer was able to examine photographs of about half of these fragments and the originals of the other half in the British Museum in London, through the courtesy of Mr. G. Lankester Harding, Director of Antiquities for Hashemite Jordan. Some forty-five different hands or variations of script were counted, indicating that as many manuscripts were apparently at one time in the cave. Yet there is a definite homogeneity of script among all the materials from the cave. One group of unpublished fragments, matted together, was secured by the Syrian Orthodox Archbishop of Jerusalem from the vandals who rifled the cave in November, 1948; and these proved to contain parts of a scroll of Hebrew prayers and two scrolls of Daniel.

The Bedouins sold the best preserved of the documents to the Syrian Orthodox Monastery in Jerusalem in July, 1947, through the help of some

* A report based on a period of research in England made possible by grants from the American Philosophical Society and the American Schools of Oriental Research in 1950–51. More detailed results of this research will appear in a volume of the series *The Dead Sea Scrolls of St. Mark's Monastery*, being edited by Dr. Millar Burrows for the American Schools of Oriental Research.

[1] A more detailed account was published by the writer under the title, Scrolls from a Dead Sea cave, in *The Christian Century*, 840–842, July 12, 1950.

[2] Report upon a fragment of cloth from the Dead Sea Scroll Cave, *Bull. Amer. School Orient. Research* **118**: 9–11, April, 1950. Crowfoot, G. M., Linen textiles from the cave of Ain Feshkha in the Jordan Valley, *Palestine Exploration Quart.*, 5–31, Jan.–Apr., 1951.

[3] Collier, Donald, New radiocarbon method for dating the past, *Chicago Nat. Hist. Mus. Bull.* **22** (1), Jan., 1951, and *The Biblical Archaeologist* **14** (1) : 25–28, Feb., 1951. Sellers, O. R., Date of cloth from the 'Ain Fashkha cave, *op. cit.*, 29.

Syrian merchants in Bethlehem, who were friends of the Bedouins. Five scrolls, making four different manuscripts, were in this collection. It was with this group of manuscripts that the writer first came in contact with the whole discovery, when on February 19, 1948, the Orthodox Syrians called the American School of Oriental Research in Jerusalem to secure information about what they had purchased, having failed to get any information from others whom they had consulted. The writer invited the Syrians to the American School to examine their scrolls. Then began a long series of events which is far too involved to treat here.[4]

The largest of the scrolls is the Book of Isaiah, consisting of seventeen sheets of leather sewn together to make a document twenty-four feet long by ten and one-half inches wide, with fifty-four columns of Hebrew writing, including the entire sixty-six chapters of the Book of Isaiah.[5] There are only a few breaks, such as those on the first three columns, in the entire manuscript (fig. 3).

FIG. 1. Interior of the cave near the Dead Sea in which the Dead Sea Scrolls were found. (Photo courtesy, G. Lankester Harding.)

FIG. 2. Jars restored from fragments found in Dead Sea cave. Each had a bowl cover like the one held here by Mr. Hassan Mamluk, an assistant in the Rockefeller Museum in Jerusalem. (Photo courtesy, O. R. Sellers.)

The second scroll was identified by Dr. William Brownlee as a commentary or Midrash on Habakkuk. It is only about six feet long, composed of two sheets of dark brown leather, badly disintegrated by time and white ants. Only the first two chapters of the Book of Habakkuk are found in the scroll interspersed with the discussion, applying the biblical prophecy to the period of the Roman conquest of Palestine.

The third document is called the "Manual of Discipline," since it includes rules and ceremonies of the sect which owned these documents as well as something of their theology. It is composed of a heavier leather, with five sheets sewn together, making the manuscript a little over six feet long by nine and one-half inches high, with eleven columns of Hebrew text.[6] Fragments of five or six additional columns of the beginning of this manuscript have since been secured by the Rockefeller Museum in Jerusalem.

[4] A detailed account of the writer's experiences in Jerusalem has been published by him under the title, The newly discovered Jerusalem Scrolls, in *The Biblical Archaeologist* **11** (3) : 45–57, Sept., 1948.

[5] Burrows, Millar, ed., *The Dead Sea Scrolls of St. Mark's Monastery* **1**, New Haven, Amer. Schools of Orient. Research, 1950. This volume includes the complete facsimiles of both the Isaiah and the Habakkuk scrolls.

[6] Burrows, Millar, ed., *The Dead Sea Scrolls of St. Mark's Monastery* **2**, Fascicle 2: Plates and Transcription of the Manual of Discipline, New Haven, The American Schools of Oriental Research, 1951.

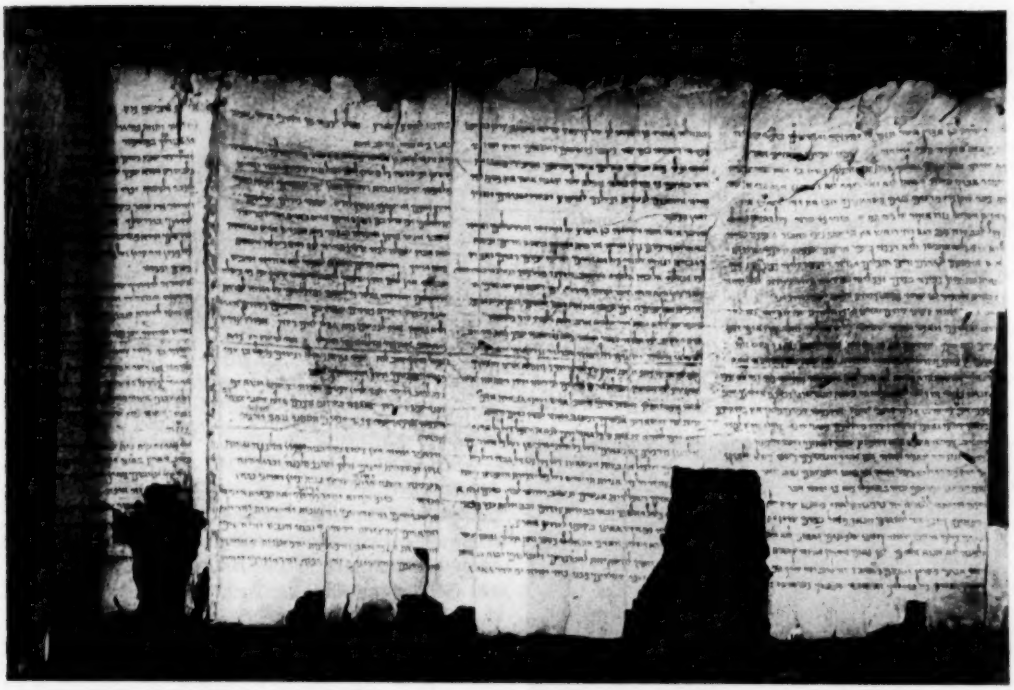


FIG. 3. The Dead Sea Scroll of Isaiah, Columns I–IV. (Photo by the author.)

The fourth document in the possession of the Syrians has not yet been unrolled, and is in a badly disintegrated condition (fig. 4). The future of this document seems to hinge on the purchase of the documents by some museum or library which can carry through on the work of unrolling the document and publishing its contents. In April, 1949, the writer succeeded in removing a fairly large fragment from the document and found that it is a narrative, purportedly written by Lamech, the father of Noah, telling, in this part at least, something of the birth of Noah. Though the name Noah does not actually appear on the fragment, comparison of the fragment with the Book of Enoch (chapter 106) makes it quite apparent that this is the general content. The most important fact about the scroll, however, is that it is written in Aramaic, a language in which we have almost no literary documents from an early period, and the current tongue in Palestine during the early Roman period.[7]

In addition to the four documents secured by the Syrian Orthodox Monastery, the Hebrew University in Jerusalem, through Dr. E. L. Sukenik, secured three additional documents in six separate sections in November of 1947. Four of these scrolls went together to make up the "Songs of Thanksgiving," written with a beautiful script similar to a number of the fragments found by the excavators. Another and less beautiful hand prepared part of the scroll.

Also among the Hebrew University documents is a scroll looking considerably like the Habakkuk commentary in structure, but which Dr. Sukenik calls the "Battle Between the Sons of Light and the Sons of Darkness," apparently an imaginary battle conceived as a part of the final days of the end of the age when righteousness will be victorious.[8]

The final scroll proved to be a very fragmentary section of another scroll of the Book of Isaiah, and to judge from its script, dates among the latest of

[7] Trever, John C., Identification of the Aramaic fourth scroll from 'Ain Feshkha, *Bull. Amer. Schools Orient. Research* **115**: 8–10, Oct. 1949.

[8] Cross, Frank M., Jr., The newly discovered scrolls in the Hebrew University Museum in Jerusalem, *The Biblical Archaeologist* **12** (2): 36–46, May 1949.

the documents found in the cave, at least a hundred years later than the other Isaiah scroll.

But the question that is occupying the attention of many scholars is how is it possible to date these documents? One phase of this question has been the area of the special research for which the American Philosophical Society has provided the writer its generous help. We have already seen how the pottery from the cave can be dated as a result of the scientific development in that field during the past sixty years. This method is not entirely satisfactory, however, since it suggests only an approximate period for the deposit; and besides, a few scholars refuse to admit that the scrolls were ever in the cave, since they were discovered by ignorant Bedouins. Such a point of view is absurd, however, for the writer has found one fragment of the "Manual of Discipline" among those found by the excavators of the cave, and the script of all the fragments and the scrolls is amazingly homogeneous.

The radioactive "Carbon 14" tests applied to some of the cloth from the cave added to the archeological evidence for a general date between the second century B.C. and the second century A.D. Some scholars believe that the only way the scrolls can be dated is by their contents: that is, their peculiarities of grammar, spelling, historical allusions, etc. But this method has led to such a wide disagreement about dating (from the second century B.C. to the period of the Crusades!), that without the help of other criteria, it would be almost impossible to resolve the problem. Furthermore, the contribution of the contents is largely to the date of the composition of the writings rather than the specific dates of the scrolls in hand. It is very probable that these scrolls are copies of earlier documents, not original compositions. Thus we come to the subject of paleography, or the study of the actual handwriting used on the documents, as a means of adding at least further evidence for the dating of the documents.

The paleographer must begin by setting up a general pattern of writing for a particular language on the basis of known dated materials, which for early Aramaic and Hebrew are not very plentiful. There are, however, the fifth century B.C. Aramaic papyri, many of which are dated to the year (fig. 5). Fortunately, many letters of this type have been found in Egypt from approximately the same time, giving considerable comparative material to illustrate the pattern of script at that time in Egypt. Knowing that the Jews adopted the Aramaic language and script sometime after the fifth century B.C., we are safe to start with these known dated Aramaic materials.[9]

Not all these letters were written on papyrus, however, for in the Bodleian Library at Oxford there is a group of twelve letters written on leather, which were found in Egypt. These can all be

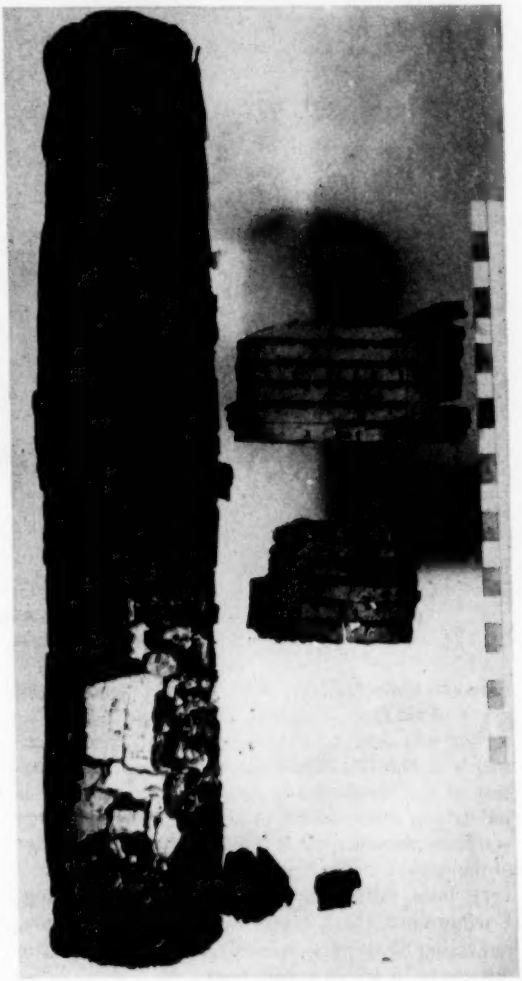


FIG. 4. The Aramaic Scroll of Lamech with two fragments. This scroll has not yet been unrolled. (Photo by the author.)

[9] The story of a new group of these papyri recently discovered has just appeared—Kraeling, Emil G., New light on the Elephantine colony, *The Biblical Archaeologist* **15** (3): 49–67, Sept. 1952.

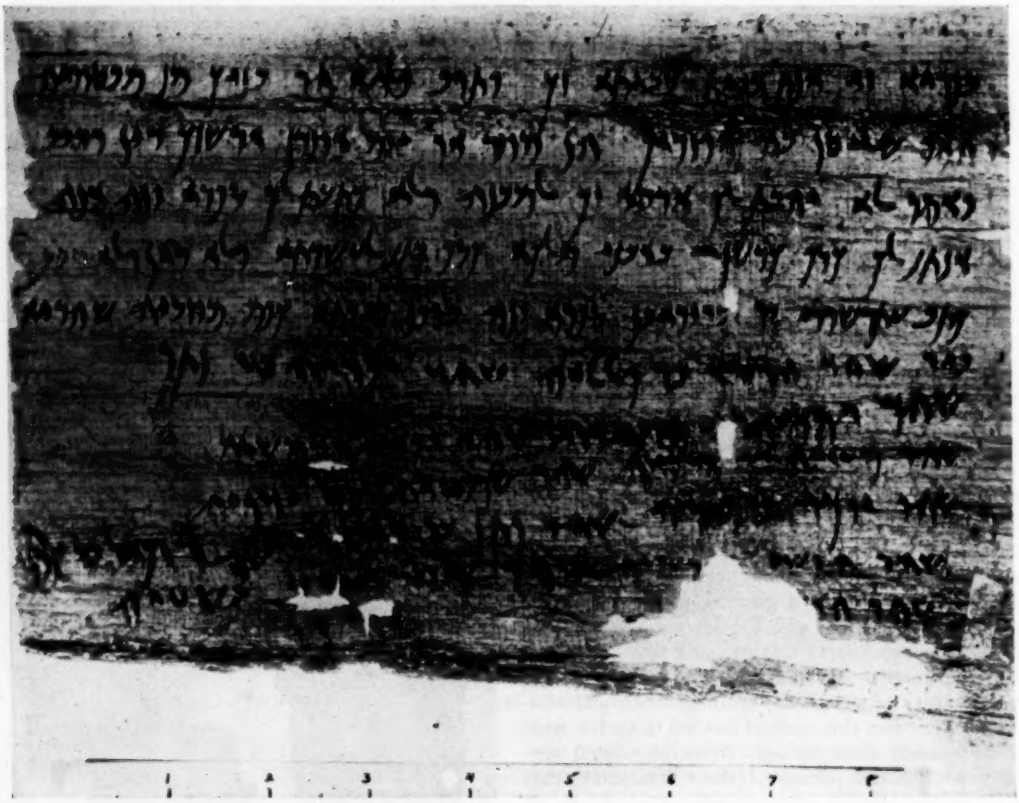


FIG. 5. Aramaic papyrus from Assuan, dated 460 B.C. (From Sayce, A. H., and A. E. Cowley. *Aramaic Papyri discovered at Assuan*, London, 1906, Papyrus C, lines 12–22.)

dated to approximately 408 B.C., with a margin of error of no more than two or three years.[10] The writer was able to examine some of these documents at the Bodleian Library, through the courtesy of the librarian and Dr. G. R. Driver, who is publishing them, and it is helpful to note that the script on these letters is almost identical with that of the papyri, indicating that these two media make very little difference in the process of writing. Furthermore, these letters were written in Persia, indicating that geographical location made little difference in script at that time.

On the other end of the scale, we can select a recent manuscript of approximately known date to see what happens to the script over a period of more than a thousand years; for example, the British Museum codex of the Pentateuch in Hebrew (Or 4445), for long considered to be one of the oldest Hebrew manuscripts of the Bible in existence. This document dates very probably from the early tenth century of our era.

The problem now is to fill in the gaps in between the two extremes of dates as far as possible with known dated materials or materials which can be dated on other grounds, until a general pattern of the development of writing can be established. The collection of papyri in the Bodleian is here again an asset, for there are found the Edfū papyri, whose contents give a pretty clear dating from the third century B.C. They illustrate certain types of basic changes in the letter forms in the direction of the tenth century A.D. manuscripts.

To narrow down the limits at the other end is more difficult for several reasons, the main one being that the greater amount of literary activity

[10] Driver, G. R., New Aramaic documents, *Ztschr. für die Alttestamentliche Wissenschaft* **62**: 220–224, 1949–50.

among the Jews in medieval times led to a much more complex pattern of writing for different purposes. Principles, therefore, which apply to the paleographic developments observable in early Aramaic-Hebrew scripts may not necessarily apply to medieval and later Hebrew manuscripts, and *vice versa*.

For this study the University of Cambridge Library is particularly valuable, since here is housed the tremendous collection of medieval manuscripts known as the Cairo Genizah. This large collection of documents was found in an abandoned storage room adjacent to an old synagogue in Old Cairo and much of it taken to Cambridge by Dr. Solomon Schechter in 1896.[11]

Almost two thousand of the more important documents from this collection have been mounted in glass and are stored in a large case where they are easily accessible for study. The writer examined most of these to see whether any of them might be from an early date or show similarities to the Dead Sea Scrolls. None of them show any paleographic evidence that would indicate a date as early as the newly discovered manuscripts.

In the basement of the library thousands of manuscripts from the same source have been organized in cardboard containers and merely catalogued according to subject matter. Altogether more than twenty-seven thousand Hebrew manuscripts and fragments in this collection have been catalogued. A perusal of these fragments is important to illustrate the problems involved in studying medieval paleography, for many different types of script are found among these manuscripts, but apparently used for different purposes. In one marriage contract there is a literary quotation at the top, written in a formal type of script, while the rest of the document is written in a more cursive style, common to such documents. This would suggest, therefore, that in dealing with the problem of the relation of medieval manuscripts to the Dead Sea Scrolls, we should limit ourselves to literary documents and their styles of writing as far as possible.

Scholars have said for many years that the oldest biblical Hebrew manuscripts date from the ninth and tenth centuries A.D. Dr. Paul Kahle, at whose home in Charlbury, near Oxford, the writer spent several valuable days, some years ago published a statement to the effect that he had found biblical manuscripts in the Cambridge collection of the Cairo Genizah which had indications of being as old as the fifth to seventh centuries A.D. Among these were fragments of scrolls of Ezekiel and the Psalms. His basis of dating was largely on the presence of a type of vowel pointing seldom found on Hebrew manuscripts, but claimed by Kahle to be early Palestinian.[12]

After checking through the mounted fragments in the Cairo Genizah, the writer found another fragment of the same scroll of Psalms which Dr. Kahle had overlooked, the reason being that there

FIG. 6. Fragment of a medieval scroll of the Psalms containing parts of Psalms 113–115. (Courtesy of the University of Cambridge Library.)

[11] Kahle, Paul E., *The Cairo Geniza*, The Schweich Lectures of the British Academy for 1941. Published for the British Academy by the Oxford Univ. Press, 1947.

[12] Kahle, Paul E., *Masoreten Des Westens* 2: 14*–45*, Stuttgart, 1930.

FIG. 7. Psalms scroll fragment, reverse, with Hebrew-Arabic palimpsest. (Courtesy of the University of Cambridge Library.)

were no vowel points on this part. The script is certainly the same, and comparing it with the ninth and tenth century manuscripts of the Bible, it would appear to be older (fig. 6). On the back of this manuscript fragment, however, there is other writing, a most unusual feature for Hebrew biblical scrolls, which were considered sacred. The script is a semi-cursive form of Hebrew, though the language is Arabic, and it has been suggested that the contents indicate that it was probably done in the twelfth or thirteenth centuries A.D.

A closer examination reveals the fact, however, that this fragment is a palimpsest, for there is writing underneath the top writing that had been erased, probably at the time that the top writing was to be done (fig. 7). The fact that this biblical fragment has been used in this way at all would probably indicate that the biblical scroll was so old and fragmentary that it would be possible to reuse it in this way, though normally it would be considered a violation of the Jewish law to do so. It would seem, therefore, that we have here an additional argument in support of Dr. Kahle's suggestion that this manuscript dates from an early time. At least it would seem that we probably have here a manuscript from several centuries before the earliest dated Hebrew manuscripts. But the type of script on this old biblical manuscript is still far removed from that of the early papyri and of the Dead Sea Scrolls!

While studying in the Cambridge University Library it was the writer's pleasure to participate in the opening of an early papyrus manuscript which has been for some fifty years in the Cambridge University Library, probably acquired from the Cairo Genizah. It is a small Hebrew codex (originally about seven and a half inches tall by about six inches wide) containing perhaps more Hebrew papyri than all the existing Hebrew papyri otherwise known. The original codex consisted of about twenty-eight folio sheets, making a quire of about fifty-six leaves. At least fourteen leaves were missing, except for their center margin strip. The extant codex is broken vertically about in half, and therefore the contents will be very difficult to decipher. Dr. J. L. Teicher, a reader in Talmudic for the University of Cambridge, is preparing the document for publication, and he says that its contents indicate an eighth century A.D. date, which therefore adds a little more to our knowledge of the form of literary Hebrew script in the early Middle Ages. The Bodleian Library at Oxford also possesses a large number of Hebrew papyri believed to date from the third to the seventh centuries A.D. They were all carefully studied by the writer, and are obviously later than the Dead Sea Scrolls.[13]

The most important piece of literary papyri in relation to the Dead Sea Scrolls is the little Nash papyrus containing fragments of Deuteronomy 6:4 and the Ten Commandments. Its date, to be sure, is not fixed; but it has a script that is similar to

[13] O. H. Lehmann of the Bodleian Library staff has attempted to show that some of these papyri are earlier than the Dead Sea Scroll of Habakkuk (DSH), (Materials concerning the dating of the Dead Sea Scrolls—I.—Habakkuk, *Palestine Exploration Quart.*, 41–47, Jan.–Apr. 1951) but his chart, Pl. X, is misleading, and his evidence by no means conclusive. The writer will discuss this problem more fully elsewhere.

that of the Dead Sea Scrolls.[14] The writer spent almost four full days in the Cambridge University Library, analyzing every detail of the script and the structure of this document and a number of points were discovered which should be published to complete its story.

The problem of publishing the Nash papyrus has been a difficult one, in view of the dark nature of its material and the very small script. Real progress in this, however, has been possible recently through processes of modern photography.

FIG. 8. The Nash Papyrus as photographed with contrast infra-red film. (Courtesy of the University of Cambridge Library.)

[14] W. F. Albright's splendid efforts to date this document are still basic to any analysis of Hebrew paleography, A Biblical fragment from the Maccabaean Age: The Nash Papyrus, *Jour. Biblical Lit.* **56**: 145–176, Sept., 1937.

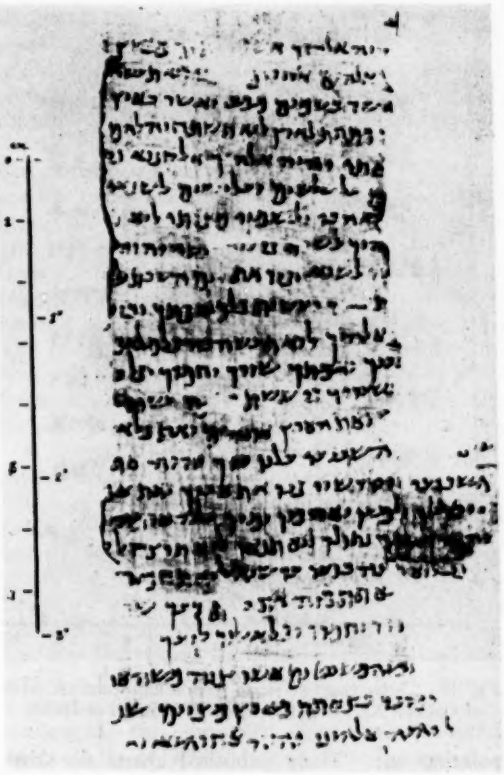


FIG. 9. A "high contrast" print of the Nash Papyrus from the same negative as for fig. 8.

After the writer discussed the problem with the Cambridge Library photographer, through the use of contrast infra-red film, he produced a result which makes it possible now to publish it clearly and resolve some of the confusion about it. It is now even possible to include the letter forms from this document on a photographically produced chart without retouching (see below). Figure 8 is an unretouched direct photograph of the document in original size.

This leads to the mention of another aspect of the writer's research which should be reported here. Many biblical scholars have been skeptical of the whole matter of Hebrew paleography, and have criticized particularly the method of drawing letter forms in charts for comparison, on the grounds that the subjective element is too liable to appear. It is true that, in drawing a certain form of a letter for a particular purpose, there is a tendency to exaggerate the feature which one is

FIG. 10. Letter chart prepared from a high contrast print of Column XLVIII of the Dead Sea Scroll of Isaiah.

pointing out. Many published charts do show that this criticism is not entirely unfounded. The writer set about, therefore, to find a method of presenting paleographic charts to remove all possibility of subjectivity.

The process consists of producing a high contrast photograph of the original document through the use of a fine grain, contrast panchromatic film, or by use of infra-red film, in the case of certain types of manuscripts (see fig. 8).

From the high contrast negative of the original, a print can be produced which removes the background, leaving the writing very clear (fig. 9). A chart can then be prepared by cutting out the individual letters and pasting them on a white card containing a scale showing the original size (fig. 10). This card is then rephotographed with "contrast process ortho" film, which allows enlargement or reduction of the final chart on a photographic print which preserves the exact forms of the letters, without any possible violation of forms (fig. 11). Publication of the finished print therefore assures absolute accuracy.

This method, of course, has certain drawbacks, in view of the fact that not all types of materials can be treated in this way. Inscriptions on stone or clay must still be reproduced by tracings, and badly defaced letters, or those on disintegrated leather, cannot be selected for such a chart. Furthermore, the high contrast tends to obscure any elements which might show the method (ductus) by which the form of the letter was achieved, but hand-drawn charts also have this latter handicap.

To return to the problem of dating the Dead Sea Scrolls by means of paleography, however, we must admit that to narrow down the limits of the history of Hebrew writing to the point where the Dead Sea Scrolls could be fixed with considerable certainty, still leaves much to be desired especially in view of the absence of literary materials of the second century A.D. It is easier to fix the upper limit of their age, since they could not possibly be as old as the third century B.C. papyri. But the lower limit is not so easy, in view of the

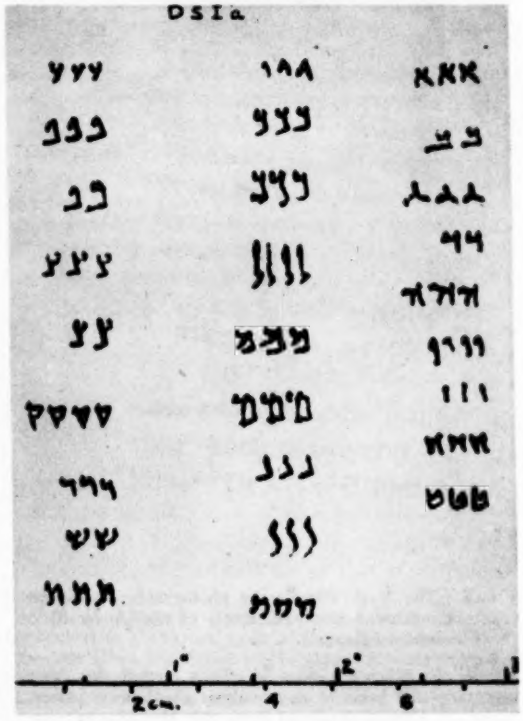


FIG. 11. Reproduction of letter chart in figure 10 by high contrast film. (Photo by the author.)

paucity of our materials from the first and second centuries A.D.[15]

One of the most important contributions to this problem, it seems to the writer, comes from the ossuary inscriptions, although the often free-hand scratching of the inscriptions into the limestone or chalk makes many of them worthless for comparison with the script of literary documents. These bone chests were a common form of disposal of the bones of deceased relatives in the first century B.C. and first century A.D., and many have been found with inscriptions in Aramaic and Hebrew, giving the names of the people whom they represent. The inability of the writer to secure a sufficient number of good photographs of this type of inscription is one of the reasons for the delay in publishing the results of the research, though it seems apparent that the script found on these ossuaries represents substantially the period in which the Dead Sea Scrolls must be placed.[16] Certain basic changes in the form of script of Hebrew seem to have been introduced in the late first century A.D. or early second century A.D., perhaps contemporaneous with the work of Rabbi Akiba, and if evidence can be established to fix the dates of these changes, we may have the conclusive evidence for the placing of the Dead Sea Scrolls prior to the end of the first century A.D.

That the Dead Sea Scrolls cover a considerable period of time of themselves is evidenced by a comparison of the many different hands represented in the total discovery. Probably the earliest of the documents found in the cave (with the possible exception of a few fragments of Leviticus in archaic script) is the St. Mark's Isaiah scroll (DSIa) which has a script not too far removed from the third century B.C. Edfū papyri. It still seems certain that DSIa must have been written near or before the end of the second century B.C. The dating of the latest fragments found in the cave will need to await the publication of the new fragments recently found in the Muraba'at caves, considerably south of 'Ain Fashkha.

It is true that the study of Hebrew and Aramaic paleography has not developed to the point where we can assign such manuscripts with confidence within narrow limits in early periods of history on the basis of the form of the writing alone, but it certainly is an important tool to set beside the other means of dating documents. Certainly it has had an important bearing on the Dead Sea Scrolls, having led those who examined the scrolls in the first place to their initial conclusion—a conclusion which has not yet been seriously challenged—that they are about two thousand years old.[17] It has also helped narrow down some of the very wide divergencies of dating derived from the study of the internal evidence only.

The writer is extremely grateful to the American Philosophical Society for making a contribution to this research, and he hopes to publish the final results soon.

[15] Reports from Palestine received about the time this paper was first presented indicate that early second-century A.D. Hebrew and Aramaic manuscript fragments have been found in some caves farther south than 'Ain Fashkha at Muraba'at and apparently not related to the Dead Sea Scroll deposits. Père Roland de Vaux, who is preparing them for publication, informs the writer that their script is later than that of the Dead Sea Scrolls! One of the documents can be accurately dated to A.D. 124. If the evidence is sufficient, it may supply the missing link that will settle the dating problem once and for all. *Bull. Amer. Schools Orient. Research* **126**: 1–2, Apr. 1952.

[16] Since presenting this paper orally, a complete set of photographs of the many ossuary inscriptions in the Rockefeller Museum in Jerusalem has been received by the writer.

[17] The writer made a preliminary attempt to date the Dead Sea Scrolls by means of paleography in his A paleographic study of the Jerusalem Scrolls, *Bull. Amer. Schools Orient. Research*, **113**: 6–23, Feb. 1949.

THE POSSIBLE SOURCE AND ANTIQUITY OF THE SLATE CHURINGAS OF WESTERN AUSTRALIA *

DANIEL SUTHERLAND DAVIDSON†

Professor of Anthropology, University of Washington

THE incised slate specimens discussed in this paper apparently belong to a class of aboriginal Australian sacred objects usually made of wood. The latter, incised with a similar range of designs, are widely distributed and their ceremonial context is well known. There are two types, the perforated and the non-perforated. The perforated examples are bullroarers and make a whir when swung at the end of a string tied through the aperture. The non-perforated may be carried about or stuck upright in the ground during ceremonies.

The slate variety is virtually unknown except for the information the specimens themselves reveal. Their area of manufacture has not been reported, hence their original use has not been observed. Only seven examples are at hand, two perforated, five non-perforated. All were collected from aborigines in localities where they were not made, where evidence of a traditional attitude toward them is lacking, and where they were regarded principally as awesome curiosities. Nor do they come from a limited area, for the points of collection are separated by distances which vary from several hundred to almost one thousand miles.

At first glance it might seem that the slate objects may be ancient relics picked up here and there at old camping places and retained as curios. However, in 1939 the aborigines in much of Western Australia denied familiarity with them and asserted that specimens were not seen around old campsites or elsewhere in the bush. Furthermore, in some cases they indicated that the known specimens were recent acquisitions obtained from neighboring tribes. In what manner the latter obtained them is of course unknown.

The provenience and antiquity of the slate specimens thus remain unrevealed by direct ethnological and archaeological investigations, but other evidence throws light on these problems. Each of the various decorative features on these objects is applied in a distinctive distribution and it is within the area they share in common that the manufacture of the slate series should be sought. These distributions furthermore have been expanding even within recent decades, hence their interrelationships have temporal as well as spacial limits. It is the purpose of this paper to detect the approximate homeland of the slate specimens by a consideration of the dynamics of the various traits involved.

The slate object in figure 1 has the appearance of a bullroarer. It qualifies as such in its physical and decorative features, but the sound produced by whirling is not very loud. It was given the author in 1939 by Mr. H. F. Broad of Wagga Wagga, east of Yalgoo, Western Australia, who had acquired it from local aborigines several years previously. To the latter it apparently had no traditional context but was regarded as a mysterious curio which came from some unknown place far to the east. The location of Wagga Wagga is shown on map 3.

This specimen throws light on an essentially similar object in the Australian Museum collected at Ooldea, South Australia, by Mrs. Daisy Bates (fig. 3*c*). The latter is described in her notes, published by Thorpe in 1923, as an old and unique plaque which served as a potent instrument for both good and evil magic and was perforated for suspension from the neck. However, Mrs. Bates called it *inma* and indicated that this term was the local name for bullroarers and other sacred paraphernalia.[1] She also asserted that the object had come to Ooldea from a northwesterly direction and assumed that it had been made in the far

* This paper represents a partial result of field studies conducted in Western Australia during 1938–1939 under the auspices of the University of Pennsylvania, the American Philosophical Society (Penrose Fund), and the Carnegie Corporation of New York.

† Deceased December 26, 1952.

[1] The word *inma* is widely distributed. The Nangumarda on the De Grey apply it to certain churingas, e.g., Davidson, 1937, fig. 55*c*. The Bardimaia at Mingenew gave it for thread cross, an object they lack but which is possessed by neighbors in the Mount Magnet area.

Northwest and had been traded southeastward past the upper reaches of the Ashburton and Gascoyne Rivers, although she gave no reasons for associating it with these particular places.

The failure of the aborigines at Ooldea to recognize the specimen as a bullroarer, if that was its original purpose, may be the result of its small size, unusual decorative features, non-traditional material, or derivation from the same direction whence came decorated pearl shells which are perforated for suspension. But since the perforation has been broken through before the object was collected, and possibly before it arrived at Ooldea, it could not have functioned as a bullroarer, or for that matter as a pendant plaque, at the time it was acquired. Be that as it may, it is to the area of its manufacture rather than Ooldea that we should look for evidence of its fundamental character.

Aside from the perforation the Wagga Wagga and Ooldea specimens can be classified by material, technique of decoration, decorative elements and relatively small size with the five other examples [2] which are essentially like wooden churingas, or non-perforated bullroarers.[3] It seems not unlikely that the slate objects have the same significance, but this question cannot be settled definitely until their use in their home area has been observed. For our purposes it is permissible to call the slate objects bullroarers and churingas.

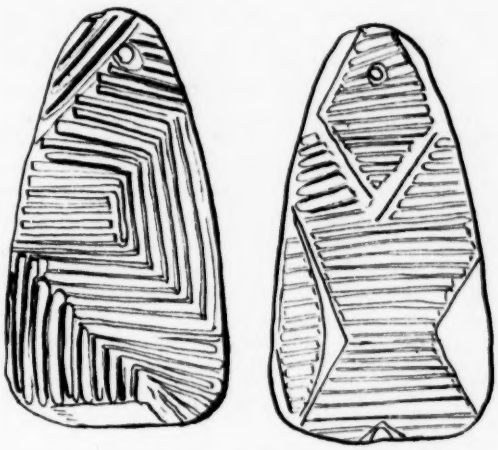


FIG. 1. Perforated slate specimen, possibly a bullroarer, collected at Wagga Wagga, near Yalgoo, Western Australia. Red ochre still adheres in the grooves.

On the basis of general similarity it is assumed that the specimens are not individually fortuitous but reflect a tradition, and therefore were made in the same area within a limited period of time. That this area is somewhere in the desert of eastern Western Australia is indicated by various types of evidence. Two examples (fig. 2*a*, *b*) are said to come from the "desert south of Wyndham," presumably the region south of the Kimberley ranges. Djaru and Kidja informants at Moola Boola in 1939 had heard that churingas of slate were used somewhere in the arid country south of the Fitzroy River, but they had never seen one. Three specimens (figs. 2*c*; 3*a*, *b*) are labeled Nullagine, which is on the western fringe of the desert, but they were not made there. Nullagine has long been a police camp where aborigines have been brought, or have come voluntarily, from a large region. Elderly aborigines residing there in 1939 were unfamiliar with slate objects and denied local manufacture or use. As already noted, the Wagga Wagga and Ooldea specimens are reported to have been traded westward and southeastward, respectively, from eastern Western Australia.

Even without inklings of directions of derivation it would be necessary to look to the uninvestigated desert country of Western Australia as

[2] Davidson, 1937: 98–104. The five are in the University of Pennsylvania Museum.

[3] The history of the word churinga is of interest. It so happened that Spencer and Gillen commenced their Central Australian investigations from the south and first encountered among the Arunta sacred wooden and stone objects which resembled bullroarers but were not perforated. These objects, as well as bullroarers, other paraphernalia, ceremonies and myths, are designated locally by the word churinga, but by a great variety of names elsewhere. In English we have such terms as bullroarer, thread cross, etc., for some of these objects, and we can conveniently group ceremonies and myths as "food increase rites," "culture hero myths," etc., but there is no suitable English term for the non-perforated counterparts of bullroarers. It would be possible to coin a phrase, and the very large specimens in Western Australia are sometimes called "dancing boards," but over the course of half a century the name churinga has become closely identified in the literature with objects of this class, regardless of location, size, decorative elements or distinctions in ceremonial use. As such it serves a useful purpose, but with a meaning quite different from its Arunta context. Comparable is the word boomerang which now designates a class of objects with a far greater range in size, form, decoration, and use than originally in eastern New South Wales. Elkin (1938: 169) has pointed out that the proper transcription of churinga is *tjurunga* or *tjuringa*. However, it is advantageous to use the latter in the specific Arunta (Aranda) context, and to retain churinga as a generic term for the class of object with which it has come to be associated and for which a separate name is so greatly needed.

the home of slate churingas for lack of evidence of manufacture or use in surrounding regions. Although these objects have not been specifically denied by fieldworkers in South Australia, Central Australia,[4] the Northern Territory, and the Kimberley region, ethnological investigations surely would have brought them to attention long since, granted that a few intrusive specimens may have escaped detection. In Western Australia their use can be denied west of the desert country. The author made inquiries from the Southwest to the De Grey River, and among Nangumarda and Karadjeri to the northwest of the desert, but all aborigines with whom slate churingas were discussed denied their use in their own as well as neighboring tribal areas. It seems quite certain that slate specimens have never been traded westward in numbers, doubtful whether other than an occasional example ever left the desert.

The decorative features of the various specimens provide the best evidence for determining the region of origin. These features include the concentric circles motif (fig. 3*b*), the interlocking key design (fig. 2*a*), the single longitudinal zigzag (fig. 1, right), crossing zigzags which enclose rhombic spaces, either outlined or filled with

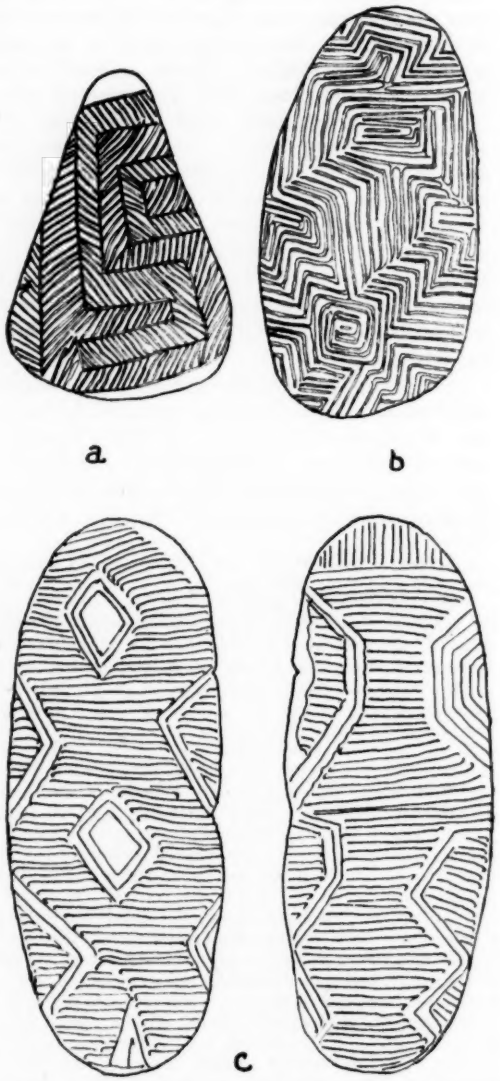


FIG. 2. Slate churingas. *a, b.* Points of collection undetermined. Each is labeled "desert south of Wyndham," presumably the arid region south of the Fitzroy River. The reverse in each specimen is undecorated. *c.* Collected at Nullagine, Western Australia, but not made there. (All are in the University of Pennsylvania Museum.)

[4] Whether the concept of using slate for churingas and bullroarers in Western Australia is derived from similar uses in the central portions of the continent is not in evidence. The stone and slate objects in the latter area are much larger and thicker and are typical only among the Arunta and Loritja, although they also are found among the Kaitish, Warramunga, Waagai, and Ilparra, the latter not being far from northeastern Western Australia. Since wooden churingas of Central Australian types have had an important influence in Western Australia, it is possible that an occasional stone specimen also was traded to the west. If so, examples have yet to be reported, although the area in which they might be not unexpected, eastern Western Australia, is the very region about which information is lacking in general.

Stone bullroarers have not been reported for the Arunta. Some Kaitish specimens are pear-shaped and have a small lump of gum at the narrow end, but Spence and Gillen (1899: 157) make no mention of a string attachment or their use as bullroarers. However, some Waagai examples with serrated edges are bullroarers. The Ilparre specimens are not described.

In spite of strict rules about storing in local sacred centers, some stone churingas have been passed on to surrounding areas, a few possibly to distant points. In the South Australian Museum are specimens collected as far from the Arunta as Lake Eyre and an undesignated place west of Gosse's Range. The possibility that some Ilparra specimens found their way deep into northeastern Western Australia and inspired a local development should not be dismissed, although present evidence does not provide tangible support for such a derivation. If one or a few Central Australian specimens are present in northeastern Western Australia the odds against finding them are great. Unless one or more are recovered in the area of manufacture of the western type, the question of outside influences for the use of slate will remain unanswered.

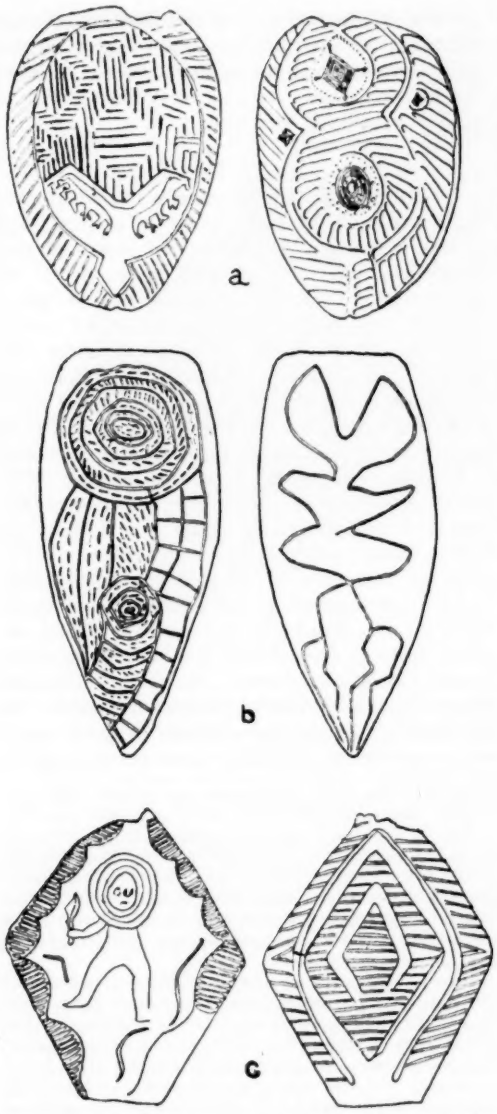


FIG. 3. Slate specimens. *a, b.* Collected at Nullagine, Western Australia, but not made there. (University of Pennsylvania Museum.) *c.* This specimen originally was perforated. Collected by Mrs. Daisy Bates at Ooldea, South Australia. All details are not shown for drawing was made from a faint photograph published by Thorpe. (Australian Museum.)

hatchings (figs. 1, left; 2*a, c*; 3*a*), and what appears to be a variant or unfinished example of the concentric rhombs motif (fig. 3*c*, right). It also should be noted that two specimens are incised on only one surface (fig. 2*a, b*), the others on both surfaces. Also shown are one large and two tiny representations of the thread cross (fig. 3*a*), and a thread oval headdress as a sketch (fig. 3*a*) and as worn (fig. 3*c*). According to Mrs. Bates the human figure in figure 3*c* holds a thread cross in each hand, but they are too indistinct to be observed in the photograph published by Thorpe, hence are not indicated.

DISTRIBUTIONS

The distributions of these traits in 1939 can be fairly well delimited. It is somewhere in the region of overlap that the provenience of slate churingas should be sought. The recent area of coextension defines only the overall limits, for slate specimens are known to be lacking in parts of it. Furthermore, as the result of the persisting vitality of aboriginal culture in northeastern Western Australia, the various traits have continued to diffuse, even in recent decades, and the area of overlap has been growing progressively larger. Since all the specimens were collected in the 1920's or before at widely separated points, it would seem that they must have been made not later than early in the present century and within the region of overlap which prevailed at the time of manufacture. The limits of the various distributions *circa* 1900 can be only approximately defined, but the general directions of diffusion of several traits since then are known, and the period of arrival of some traits at certain points can be roughly estimated. It should be recognized that specific dates of diffusion are seldom available. Ethnological records are made only from time to time; hence the date of adoption of a trait, or of the arrival of a new type of traded object, usually can be assigned only to an interval. For our purposes approximate dates are convenient for comparisons, relative chronology and perspective. In many cases they can be given only to the nearest quarter-century. Thus in one place *ca.* 1900 may mean 1890 or even earlier, at another 1910 or even later. But since we are concerned with points often separated by a few hundred miles or more, such a margin of error is not of major consequence for the type of problem involved.

In considering the problem of slate specimens attention should be given to—

1. Bullroarers.
2. Churingas.
3. The custom of incising either one or both surfaces.
4. The concentric circles motif.
5. The interlocking key motif.
6. The longitudinal zigzag motif.
7. The concentric rhombs motif.
8. The thread cross.
9. The thread oval.

1. BULLROARERS

Bullroarers, basic to churingas, occupy almost the entire continent. That they were not a possession of the earliest aborigines is indicated by their lack in such peripheral areas as Tasmania, Melville, and Bathurst Islands and the adjacent mainland, their reported relatively recent spread into the northern Kimberley region,[5] and their very recent arrival in the extreme Northwest[6] (map 1). Their antiquity in the Southwest is uncertain.[7] It would seem that they have been present in the interior of northeastern Western Australia for a minimum of a few centuries, but a greater antiquity is not suggested, nor is it denied, by the indications of recent diffusion into the nearby peripheral locations.

2. CHURINGAS

These objects are essentially non-perforated bullroarers. Since they cannot be swung, length is not a practical limitation and some Western Australian specimens exceed fifteen feet. For our purposes two major types can be distinguished, a Central Australian type incised on both surfaces with concentric circles as the most characteristic motif, and a Western Australian type incised on only one surface with the zigzag as the most widespread but not the only design. In the same regions bullroarers are similarly decorated.

Churingas are found in a much smaller distribution than bullroarers, but nevertheless an extensive and contiguous one (map 1). The overall limits, which in some but not all regions coincide with the boundary of circumcision and/or subincision, are marked by the Katherine and Roper Rivers in the Northern Territory (*ca.* 1930), extreme western Queensland (*ca.* 1895), northern South Australia (*ca.* 1895), the Great Australian Bight near the Western Australian border,[8] the Southern Cross area,[9] the Mount Magnet area,[10] the central Murchison and upper portions of the

[5] Elkin (1932: 298) reports that bullroarers were not made by the northern Kimberley tribes until recently, but he makes no suggestion as to the time of introduction. The problem is complicated by the fact that the bullroarers in this region are unlike those which have been imported in recent times from the south. Most specimens are not incised, but decorated with painted dots or bands as in the Northern Territory. If they and the non-perforated examples have been derived recently from the east, evidence should be obtainable. If the types from the south are to be adopted locally the incising technique will have to be acquired.

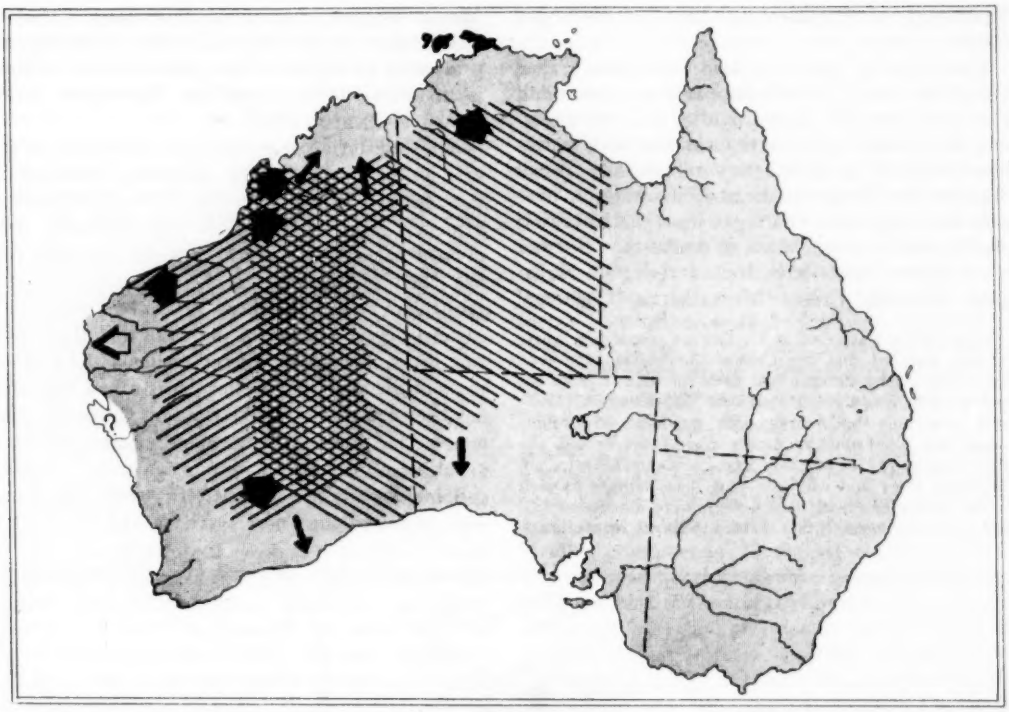
[6] In the Northwest, according to old European settlers, the Baiong at Minilya Station received their first bullroarers from Lyons River tribes about 1910. It appears that these objects arrived as curios ahead of knowledge of sacred context, for they were used at least in part as playthings and as such were taken up with enthusiasm. The nearby Talainji, Burduna, Ingarda, and Kurama asserted in 1939 that the bullroarer was a relatively recent acquisition. It reached the Ingarda from the east, that is from tribes up the Gascoyne River, the Kurama from the north, or via the Fortesque River. South of the Wooramel River along the west coast the situation is obscure. Bullroarers have not been specifically denied but the museums seem to lack specimens from this area.

[7] According to Bates (1938: 124) the Bubbelmun, the name she applied to southwestern aborigines in general, bought bullroarers from other tribes and permitted their children to play with them. That some incised specimens were imported from tribes to the north and northeast, possibly as curios or playthings, seems not unlikely, for cultural influences were predominantly southwestward in this region. However, Hammond (1933: 34, 36) implies that unincised bullroarers, nine to fourteen inches in length, and with serrated edges, were widespread in this region. In the Busselton-Augusta area of the extreme Southwest the local bullroarers, up to ten inches in length, were undecorated but nicely polished, according to Mr. A. J. Bussell, age eighty-four in 1939. Since undecorated specimens have not been reported from adjacent inland tribes it would appear that the plain bullroarers were not the imported objects mentioned by Mrs. Bates. However, the apparent weakness in esoteric context in the southwestern region does not suggest that bullroarers have much antiquity there.

[8] Elkin (1938: 170) apparently is referring to the Central Australian type of churinga. Further west, about the turn of the century, aborigines from the Coolgardie area, visited as far south as Esperance, performed ceremonies, and showed sacred paraphernalia, presumably all of western types. An informant from Esperance professed the shame and inferiority felt in his group as the result of the lack of authority to circumcise and subincise the young men and to own objects associated with these ceremonies. According to his testimony churingas were lacking among all coastal tribes west of Esperance.

[9] The aborigines around Southern Cross in 1938 considered typical Murchison designs as their own and gave the impression that the local use of churingas was the result of influences from adjacent northern neighbors.

[10] Churingas with Western Australian designs of the zigzag type are found in the Mount Magnet–Cue area, but it is not clear whether they were used further to the west.



MAP 1. Distributions of bullroarers and churingas. Bullroarers present. Bullroarers lacking (Tasmania not shown). Bullroarers recently introduced. Churingas of incised Central Australian type. Churingas of incised Western Australian types. Directions of recent spread of the various types. Recent introduction of churingas to neighboring areas.

Gascoyne and Ashburton Rivers,[11] and the northern coast of Western Australia east of the Fortesque River, excluding apparently until recent times the northern parts of Dampierland and the northern Kimberley region.[12]

The Central Australian type occupies the larger distribution. In 1930 specimens apparently were made as far west as a line running approximately from La Grange to Laverton[13] (map 2). Many peripheral parts of this distribution have been attained by quite recent expansion and in some places diffusion may be continuing today.[14] On the northwestern periphery the Karadjeri and their southern neighbors apparently did not adopt this type until after the turn of the century, the

[11] In the Northwest churingas are lacking among the Talainji, Baiong, Maia, Ingarda, Targari, Kurama, Noala, Warienga, and Jiwali, and undoubtedly other tribes in this region. The Janjima and Injibandi possess them as the result of influences spreading down the Fortesque River. They in turn introduced specimens to the coastal Mardudhunera and Ngaluma in fairly recent decades. In 1939 an old Mardudhunera stated that he first saw churingas some "twenty" years previously, but his assessment of time should not be taken too literally.

[12] For Dampierland in 1905 Klaatsch (1907: 579) states that bullroarers but not churingas were found north of Broome. Elkin (1936: 202–203) describes the bullroarers of the Bard tribe in the northern tip of the peninsula for 1928, but makes no mention of churingas. However, a churinga labeled Sunday Island is in the University of Pennsylvania Museum (Davidson 1937: fig. 57*d*) but probably comes from the King's Sound mainland. It is incised on both surfaces with herringbone designs, over which are painted bands. In the Western Australian Museum are specimens with herringbone patterns labeled Beagle Bay and Lombadina. Many of these specimens may be trade pieces. If so their area of manufacture has not been determined. See footnote 5.

[13] Elkin (1938: 170) defines the La Grange-Laverton line but does not give details.

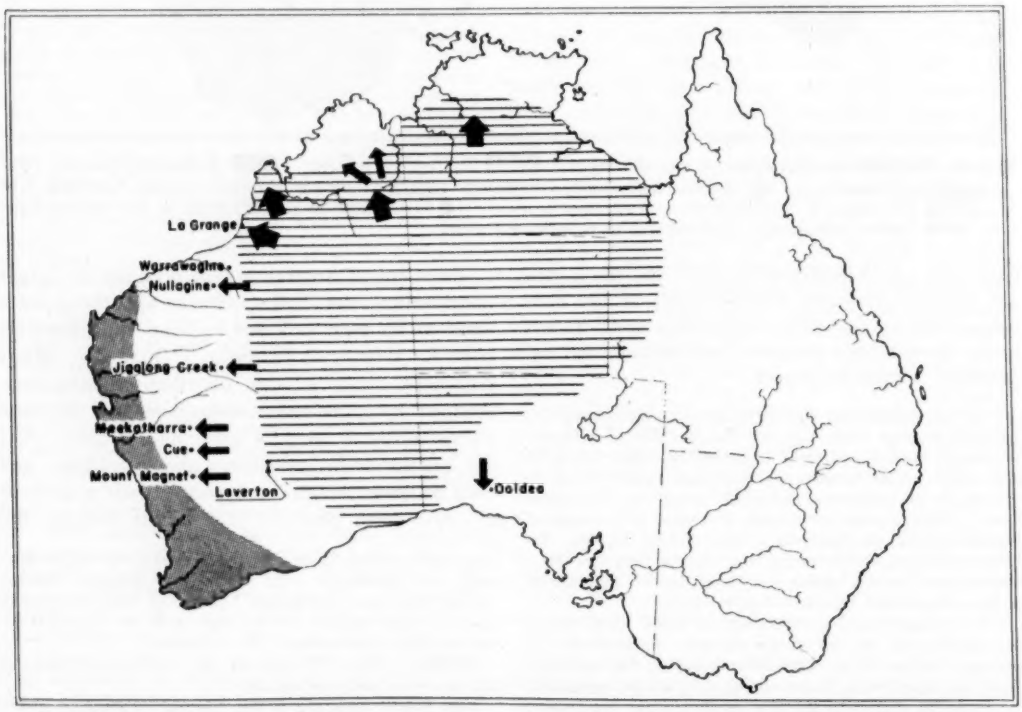
[14] In South Australia a few Central Australian specimens had reached Ooldea before 1930. Influences may have become much stronger during the drought in the early 1930's when many aborigines from Central Australia made long visits.

Nyul-nyul in Dampierland between 1905 and 1929.[15]

Specimens of this type also have been traded out of the area of manufacture in many directions. For some time the Djaru, Kidja, and other eastern Kimberley tribes have sent numbers northward beyond the Kimberley ranges. In recent decades the Nangumarda at Warrawagine have been receiving these churingas from the Karadjeri and the southern neighbors of the latter. Further south these objects have been arriving at Nullagine, Jigalong Creek, Meckatharra, Cue, and Mount Magnet, and strays have been found at many other places. As the result of the disintegration of aboriginal culture little interest in them is manifest in the south, but importance is attached to them in the north.

The Western Australian type is found as far east as the border of the Northern Territory, if not that of Central Australia. The distributional evidence suggests that this type formerly was present in much of eastern Western Australia and has been giving way to the advancing Central Australian type. However, it is only in the northeastern desert portion of the state that this trend can be confirmed by direct evidence. In other parts of the state the Western Australian type also has been expanding its distribution within the historic period, although partly as the result of Europeanization little information is available. In 1939 specimens with the key pattern were still being sent eastward and northward in the Kimberley

[15] In 1939 a Karadjeri at La Grange, about fifty years of age, asserted that the Central Australian type with concentric circles designs had been introduced from the east when he was a young man. In 1905 Klaatsch (1907: 648) saw only bullroarers with quadrilateral designs among the Nyul-nyul at Beagle Bay, Dampierland. In 1929 Porteus (1931: 36) found typical Central Australian churingas there and was told that these objects as well as the ceremonies with which they were associated had come from the interior, that is from the south or southeast.



MAP 2. Distribution of the concentric circles motif. Area in which the design was applied in 1930 (or formerly). Directions of relatively recent infusion. Recent introduction of churingas with this design to neighboring areas. Approximate area in which the design was not recognized by aboriginal informants except a few experienced travellers.

region. In the Northwest churingas with the zigzag design have been introduced down the entire length of the Fortesque River in recent decades.[16] Until about 1900 similar specimens were being shown as far south as Esperance by aborigines from Coolgardie, although it is uncertain whether these objects were left there.

It is in the desert south of the Kimberley ranges that the Western Australian and Central Australian types are known to co-exist. Certain designs on the former are becoming obsolescent and the Central Australian elements have become or are becoming dominant. The predominance of older western designs on the slate specimens suggests that at the time these objects were made the key and concentric circles motifs had not yet become prominent in the area of manufacture and may have been of recent introduction.

3. THE CUSTOM OF INCISING EITHER ONE OR BOTH SURFACES

The distributions of these two traditions meet in eastern Western Australia, hence it is in this region that interinfluences should be not unexpected. It would be possible for interchange to be expressed in various ways. Central Australian designs could be incised on one surface of a plano-convex western specimen. Western motifs could be placed on both surfaces of the Central Australian bi-convex type. Or the designs of the two traditions could be applied side by side. How common such interplay may be remains to be determined. However, some Kidja and Djaru wooden churingas show both central and western motifs, and five of the slate objects are decorated on both surfaces, four with western motifs (figs. 1; 2*c*; 3*a*, *c*), the fifth with one surface exhibiting atypical Central Australian concentric circles designs filled with hatchings in western style, the reverse with a non-descript and apparently unfinished decoration (fig. 3*b*).

4. THE CONCENTRIC CIRCLES MOTIF

This design is found throughout the distribution of the Central Australian type. It also is painted on persons and various ceremonial objects, but too few data are at hand to indicate the distributions of these associations. In the central regions it also appears in rock paintings.

The area of application to bullroarers and churingas has been expanding (map 2). The design was adopted by the Karadjeri, their southern neighbors, and the Nyul-nyul apparently early in this century. The Djaru in the eastern Kimberley region asserted that the design (*únari*) reached them from a southeasterly direction with their *króga* type of churinga.

The distribution of traded specimens with this design also has been expanding. The Djaru and their neighbors have been sending churingas northward through the Kimberley ranges. Within a few decades after the Karadjeri and their southern neighbors had acquired the motif, early in this century, they were introducing their own specimens westward as far as the De Grey, and random specimens probably had been passed on in earlier times. Further south churingas have been received for some time at Nullagine, Jigalong Creek, Meekatharra, Cue, Mount Magnet, and other places. In 1939 the aborigines at all these places associated the motif with tribes to the east and none had begun to apply it. Further west some tribes were familiar with the design but had not received specimens. In the far northwest, west, and southwest the motif was not recognized.[17]

The export of churingas with the concentric circles motif westward and northwestward along almost a 1,000-mile front, and the strength of the diffusion following in their wake, suggest that the momentum was well established before the beginning of the twentieth century, at which time specimens were nearing, if some had not already been received by, the Karadjeri and southern Dampierland tribes. These considerations do not suggest that the Central Australian type enjoys much antiquity south of the Kimberley ranges, or that it has co-existed for long with the various Western Australian features associated with slate and wooden churingas.

5. THE INTERLOCKING KEY MOTIF

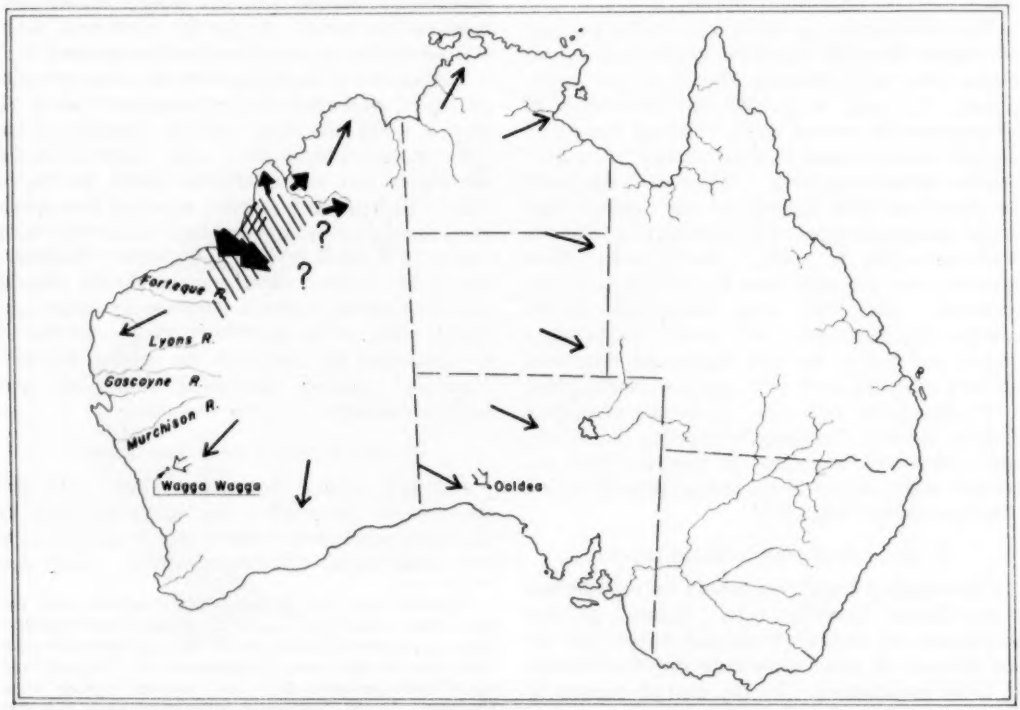
Although known throughout almost half the continent as the result of the extensive trade in decorated pearl shells, this design is applied in a very small area (Davidson, 1949). Only the

[16] See footnote 11.

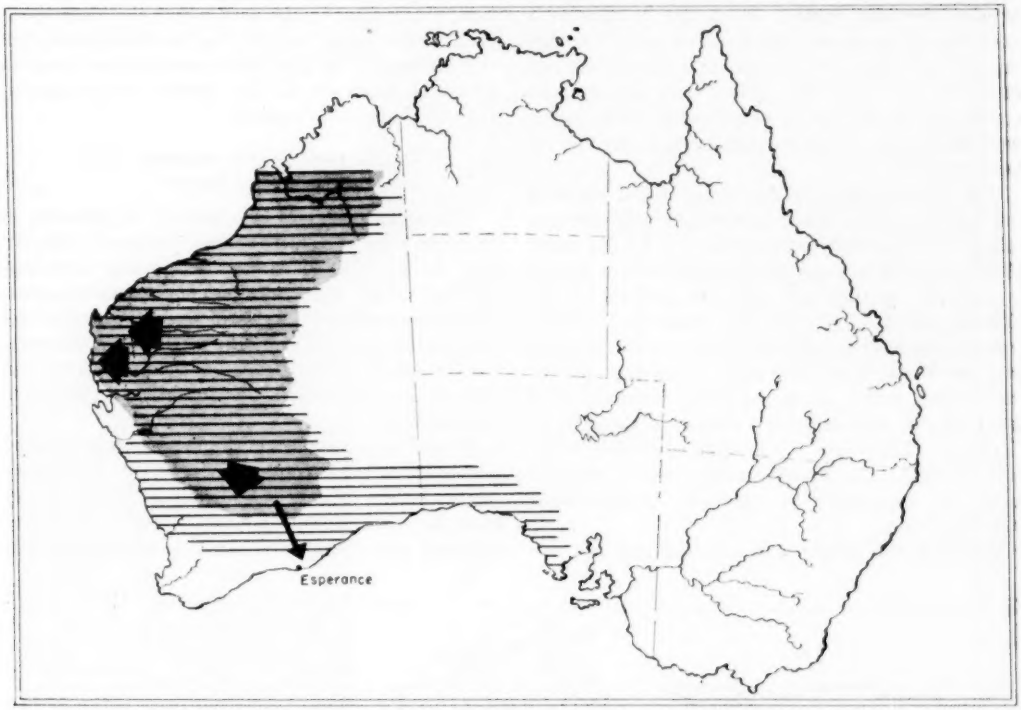
[17] Further west the Injibandi and Panjima were the only tribes along the central Fortesque River familiar with the concentric circles motif. They associated it with tribes far to their east, Nangumarda or Targudi, and stated that specimens were not received in their area. Elsewhere in the Northwest informants were not met who recognized the design other than an occasional aborigine who had traveled in the employ of Europeans. In the far west the Bardimaia ay Mingenew and Mullewa were not familiar with it, nor were the aborigines in the Southwest reared or residing at Kellerberrin, Quirading, Gnowangerup, and Esperance.

Karadjeri are reported to have incised it on pearl shells and shields. It appears very occasionally on boabab nuts decorated at nearby Derby. It is to bullroarers and churingas that its application is most widespread, from the lower Fitzroy to the De Grey area, and inland as far as the Targudi in the west, and for an undetermined distance in the east (maps 3 and 6). This distribution is not of long standing. Apparently around the end of the nineteenth century the Nangumarda were importing specimens with this design from the Karadjuri, the Targudi from desert tribes south of the Karadjeri, if they had not already adopted the design at that time. By the 1930's this motif had become the dominant churinga design of these tribes and had almost completely replaced the zigzag pattern which still prevailed to the east and south. East and southeast of the Karadjeri the situation is obscure, for the boundary of the area of application has not been defined. However, an eastward movement is indicated in so far as traded specimens are concerned. In 1939 the Djaru asserted that they had been receiving an increasing number in recent times via trade route south of the Fitzroy River. Pearl shells with this design reach them via the Fitzroy River route.

It should be noted that the key pattern, like other churinga designs, cannot be applied without authority and that the aborigines have great respect for these rights. Whether the aborigines at Derby received permission to apply it to boabab nuts, or stole it from a churinga, shield or pearl shell, is unknown, but there are no indications of indiscriminate borrowings in other regions, even in the distant places where pearl shells have been received but where knowledge of ownership and context of the pattern are lacking. It seems unlikely that the design on the slate churingas in fig-



MAP 3. Distribution of the interlocking key design. ▨ The original center of application to pearl shells (Karadjeri tribe). ⟵ Distant points to which decorated pearl shells have been traded as curios. ▧ Approximate distribution of the application of the key design to churingas in 1939. ⟵ Directions of spread of churingas with the key design late in the nineteenth century. ⟵ Introduction of specimens to neighboring tribes in recent decades. ⇦ Directions of derivation of the Wagga Wagga and Ooldea slate specimens which show different designs (figs. 1 and 3*c*).



MAP 4. Distribution of the incised zigzag design. On spearthrowers. On bullroarers and churingas. Directions of recent diffusion. Specimens shown by visitors from Coolgardie about 1900.

ure 2*a* was applied without authority. The style is typical of wooden churingas in that the spaces between the lines are filled with hatchings, a feature found only occasionally on pearl shells. On the latter the key motif usually is expressed by sets of two or more parallel lines.

The evidence indicates that the distribution of traded churingas with the key pattern has been expanding eastward to reach the Djaru a few decades ago and suggests that the area of application also has been extended eastward in recent decades to approach Christmas Creek. Whether expansion southward and southeastward has continued in recent times is unknown. It is quite possible that until about the middle of the nineteenth century the Karadjeri were the sole possessors of the key motif.

6. THE LONGITUDINAL ZIGZAG MOTIF

This design is distributed in a great arc from northeastern Western Australia, south of the Kimberley ranges, to Spencer Gulf, South Australia (map 4). Of the standard varieties the single zigzag is the most widespread. Two parallel zigzagging elements also are common. Three parallel zigzags seem to be centered in the Murchison area. Crossing zigzags are found from central Western Australia to the northeastern part of the state south of the Kimberley ranges.

In most of its distribution the zigzag is applied to spearthrowers. In much of Western Australia it also appears on bullroarers and churingas. As a bullroarer design the motif is new in the far Northwest, having accompanied this object in its relatively recent spread into this area, where it still is considered the only proper design for bullroarers, churingas being unknown.[18]

In certain other areas the zigzag is now obsolescent. The Djaru still applied it in 1939 but attached growing importance to the application of the concentric circles motif and to the importation of churingas with the key pattern. The Karadjeri considered it an old design which had fallen into

[18] See footnote 6.

almost complete disuse. Both the Nangumarda and Targudi asserted that it was a proper design but old fashioned and seldom applied since the key motif became dominant. They were aware of its continued importance in neighboring areas to the west and south where the key design had not yet been adopted.

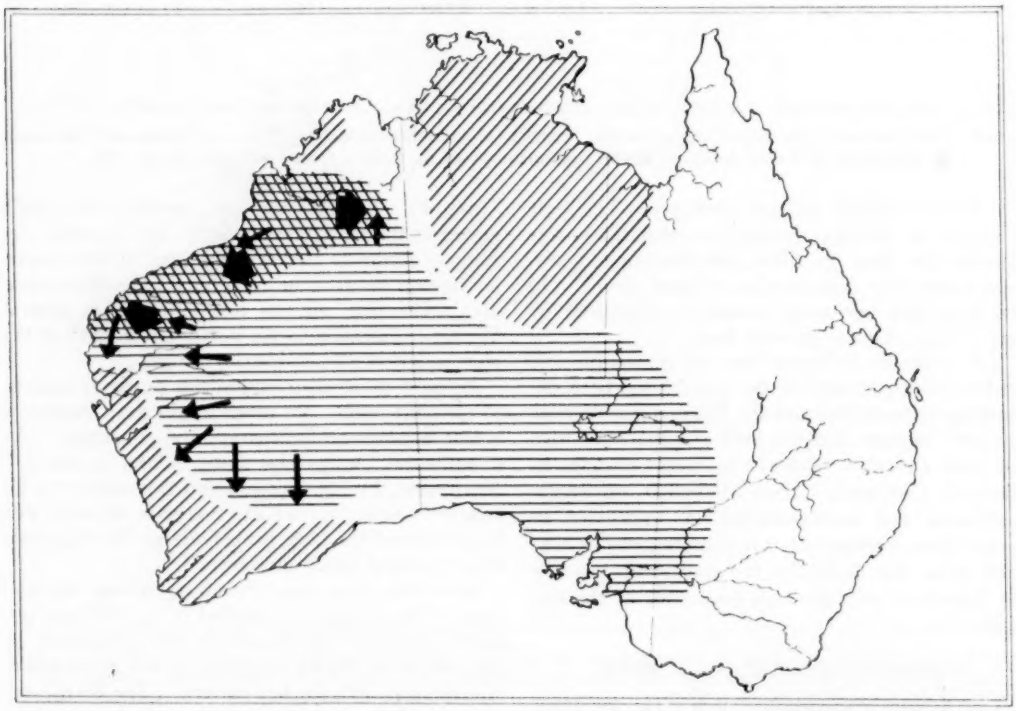
The eastern limits of the zigzag in the region a few hundred miles south of the Kimberley ranges cannot be indicated with certainty. In the early 1930's a Djaru saw spearthrowers with this design in both the eastern and western portions of the Canning Route. There is no reason to doubt its continued use as a churinga design at the western end, but it may have been replaced by the concentric circles motif in the eastern portion. The latter is the only churinga design mentioned by Carnegie (1898: 347) who passed through the area in 1897. As already noted, such a replacement is suggested by general distributional considerations.

Whether the single and crossing zigzags are now obsolescent in the area where the slate specimens were made remains to be determined, but there seems to be a general tendency for them to give way wherever the key pattern or the concentric circles motif is applied.

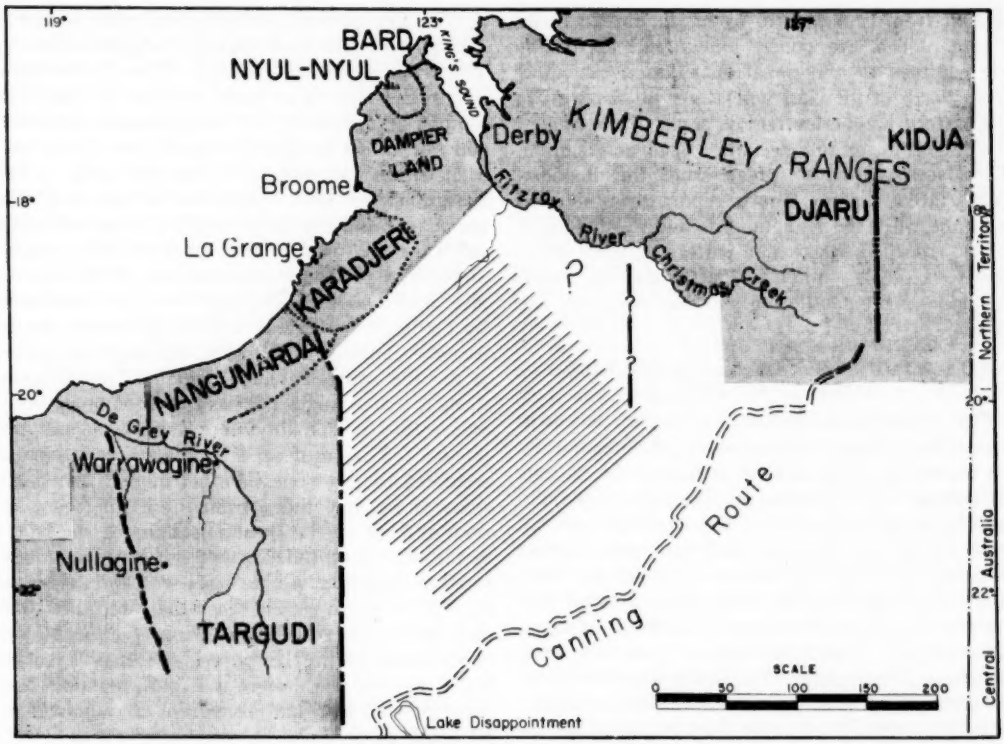
7. THE CONCENTRIC RHOMBS (AND SQUARES) MOTIF

The appearances of this pattern or patterns in Western Australia are widely scattered (Davidson, 1937: 119). Information is not abundant and the design may be obsolete in various areas. There are two main styles of execution, either by lines, or by spaces filled with hatchings in alternating directions. Bullroarers and churingas are the objects most frequently decorated, spearthrowers occasionally.

There is no evidence that concentric rhombs and squares were known north of the Kimberley ranges before incised decorations were introduced from the south in recent times. There are many museum specimens from south of the ranges but



MAP 5. Distributions of thread cross and thread oval. Thread cross present. Thread cross lacking. Directions of recent diffusion of thread cross. Thread oval present. Directions of diffusion of thread oval.



MAP 6. Northeastern Western Australia. Slate churingas denied or their lack inferred from other evidence. Potential area somewhere in which home of slate churingas is indicated. —— Western limit of application of concentric circles design in 1939. — — — Western limit of intentionally introduced churingas with the concentric circles motif in 1939. — ? — Application of key motif to churingas not east of this line, if that far. —··· Eastern limit (?) of introduced churingas with key motif in 1939.

until the area of application has been delimited it is impossible to indicate which are trade pieces. The patterns were present among the Djaru in 1939, the Nyul-nyul at Beagle Bay, Dampierland, in 1905 (Klaatsch, 1907: 648). The Nangumarda asserted that concentric rhombs were important among the Kariera and that they, themselves, applied it to churingas, and less frequently bullroarers, used in the *karaŋada* ceremony which was adopted from the Karadjeri. Among the Nangumarda the design seems to be relatively old, the ceremony less so.[19]

The appearance of the concentric rhombs motifs on the slate specimen in figure 3*c* is consistent with its association with wooden churingas in other part of northeastern Western Australia.[20]

[19] Piddington (1932: 81–82) discusses the Karadjeri *kuraŋada* ceremony.

[20] Bates (Thorpe, 1923: 60) considers the design on the Ooldea slate specimen as phallic in character, but presents no supporting testimony from aboriginal informants. The original meaning can be determined only in the area of manufacture.

8. THE THREAD CROSS

The simple thread cross is widely distributed in Australia (Davidson, 1951). It too has been spreading in recent years, but in areas which do not pertain to the slate churinga problem (map 5). Since it is associated ceremonially with churingas in many areas, its portrayal on slate churingas is understandable, although there are no reports of wooden specimens so decorated. The three examples in figure 3*a* are obvious. Those on the specimen in figure 3*c* are not shown.

9. THE THREAD OVAL

The thread oval is worn around the face and consists of a round or oval foundation from which radiate several sticks to support the string-work

wound from arm to arm as in the thread cross. These objects are poorly known but apparently are confined to a regional distribution extending from north of the Gascoyne River on the west to the eastern Kimberley region, south of the ranges, and inland for an undetermined distance (map 5). They seem to have spread from the Broome-Derby area, for the Djaru report them derived from the west, the Nangumarda and northwestern tribes, from the east. The pattern of their diffusion thus seems similar to that of the key motif, but data on their antiquity and rate of spread are lacking.

POTENTIAL AREA OF SLATE CHURINGA MANUFACTURE

The region shared in common by the concentric circles, key, zigzag, and concentric rhombs motifs, the thread cross, the thread oval, and the custom of incising both surfaces of churingas and bull-roarers is not large. Its dimensions in recent years are greater than in 1900 and some expansion may be continuing today. In 1939 the area in which all these traits were fully adopted extended from the northern sector of the La Grange-Laverton line to an undetermined boundary near Christmas Creek (map 6). If the respective distributions were to include the localities in which some traits were known only from imported specimens, the overlap would embrace Nullagine and the De Grey River on the west and the Djaru country on the east. In either case the undefined southern limits would have coincided with the southern boundary of either the thread oval or the key design, whichever was nearer. That these limits approached parts of the Canning Route seems not unlikely, but if they crossed it at one point or another before 1939 or subsequently, the evidence remains to be collected.

Since the slate specimens were made at the very latest early in the twentieth century, the importance of the 1939 area of overlapping distributions is in the light it throws on the rate of expansion since 1900 or thereabouts, for it apparently was not until after the turn of the century that the concentric circles motif was adopted by the central Dampierland tribes, the Karadjeri, and inferentially their southern neighbors as far as Lake Disappointment, although all probably had been importing specimens for at least a few years prior to adoption. It also would appear that the Djaru have not been applying this design for long, but the date of its adoption is uncertain.

The key pattern probably was known east of Christmas Creek from traded churingas before the end of the nineteenth century. How far east stray specimens may have been received at that time cannot be indicated, but these objects apparently did not arrive in quantity among the Djaru until well within this century. The application of this design has not been reported as far east as Christmas Creek and any early tendency to spread southeastward may have been slowed and possibly stopped by the eastward expansion of the concentric circles motif. The key was not mentioned by Carnegie in 1898 as a churinga design in the eastern Canning Route area, although his silence is not the equivalent of a denial. Traded specimens if scarce might not have been obvious, but if scarce they presumably were neither old nor important. Furthermore, this area seems rather far southeast to have received other than an occasional stray, if we may judge from the more recent arrival of these objects in numbers among the Djaru in the same longitude further north. The southwestern boundary of churingas with the key motif in 1900 is unknown, but since the Targudi at that time seem to have been receiving specimens from tribes south of the Karadjeri, and may have already adopted the design, it is quite possible that these objects also had been sent in numbers as far south as the Lake Disappointment area.

The concentric rhombs (and squares) and zigzag designs seem to have become obsolescent in all areas where the concentric circles and key motifs have been adopted for any length of time. Whether they formerly were applied to churingas in the eastern Canning Route area is not in evidence, but is not unlikely. Neither is mentioned by Carnegie, but it is possible that they had been replaced by the concentric circles design before 1897. Both were applied by the Djaru in 1939. The concentric squares motif was seen at Alexander Springs, 250 miles south of the central Canning Route, in 1896 by Carnegie (1898: 348–349). Since the zigzag is widely distributed as a spear-thrower design its appearance on this type of weapon in the eastern Canning Route area in the 1930's is not necessarily an indication of a former application to churingas, although it possibly reflects a long local association with spearthrowers. Its declining importance further north, where the concentric circles motif has become dominant in recent times, warrants the suspicion that it formerly was applied to churingas in the eastern Canning Route area.

Around 1900 the region in which all the traits were fully adopted, and therefore available for application to slate churingas, included the area southeast of the Karadjeri, the country along the Fitzroy River possibly almost as far east as Christmas Creek, and the interior toward the Canning Route. If it is granted that some designs may have been copied from traded specimens without authorization, a moot point, the overlapping distributions probably would have included the Karadjeri and tribes to the south possibly to Lake Disappointment, the area north of the Fitzroy, the country east of Christmas Creek for an undetermined but presumably not great distance, and the approaches to the eastern and central portions of the Canning Route. However, as is shown by the shading on map 6, we can define as negative not only the western and northeastern extensions, for which the Karadjeri, Djaru, and others have denied slate churingas, but also the area along the Fitzroy River where slate specimens have not been reported among the many sacred objects collected by Europeans during a long period of contact with the aborigines. On the south, for want of specific evidence, the Canning Route again is a convenient designation for a boundary of uncertain whereabouts.

On the basis of these considerations it would appear that around 1900 the place of manufacture of slate churingas was somewhere southeast of the Karadjeri, south of the Fitzroy River, west of Christmas Creek, and north and northwest of the Canning Route.

ANTIQUITY OF SLATE CHURINGAS

For the period prior to 1900 specific evidence is lacking. Yet there are reasons for suspecting that the antiquity of slate churingas is not great. Final attention to this question must await additional ethnological data, and may depend on archaeological investigation, but a relatively recent development is suggested by the inference that the concentric circles motif and the key design had not spread to an area in common with the other decorative details until sometime in the nineteenth century. The impetus in their spread since 1900 indicates in each case that a momentum had been gained before that time. But even if the rate of expansion had been slower in the last quarter of the nineteenth century, the potential area of manufacture in 1875 would have been considerably smaller than in 1900 when the approximate east-west axis may have been about two hundred miles. The important consideration is the fact that the key and concentric circles designs are derived from opposite directions, for when two distributions expand, each into the territory of the other, the extent of overlap increases at twice the mean rate of the two. Given a few facts the problem is a matter of arithmetic, for if the major dimensions and the two rates are known, the date and place of convergence can be determined.

In considering the spread of objects, a distinction should be made between the random circulation of curios or odd specimens traded beyond the limits of original context, and the intentional introduction of sacred objects to neighbors for the purpose of influencing their ceremonial complex, and both should be recognized as different from the diffusion of knowledge which provides the context. Knowledge of use may not keep pace with stray specimens traded great distances, although elementary information may accompany the first churingas or bullroarers intentionally introduced into a neighboring area. It is of course quite possible that an object's reputation may precede it into an adjacent area, but inquiry on this question revealed that when such knowledge was present it had been gained during visits to areas where specimens had been seen.

Little is known about the trade in curios or the attitudes toward such objects. Museum labels on many ethnological and archaeological specimens seem to reflect a general assumption that objects collected far from their respective areas of manufacture had the same context as in the home country. Such a conclusion may be justified in most cases, but it should not be taken for granted. It seems clear that the purpose of sacred objects is not obvious, but it also should be recognized that the use of utilitarian implements is not always apparent. It will be recalled that hardly two centuries ago it was difficult to convince Europeans that archaeological specimens were not thunderbolts, and even today archaeologists do not agree on the purpose or manner of use of many types of artifacts. In this respect it is of interest to note that some aborigines in the far Northwest confused intrusive semi-polished stone ax-heads, found occasionally in the bush, with upper grinding stones. The Nangumarda at Warrawagine and the Targudi and others at Nullagine in 1939 had received from time to time a few-pressure-flaked Kimberley stone points which they designated in English as "glass-spears," a widely used term. The name would suggest a knowledge of use, but these people

professed their ignorance in this respect when specimens were first received, and even in 1939 they were quite astonished to see photographs of these objects actually mounted as spear-heads. The Worora of the western coast of the Kimberley district do not make or use boomerangs. They are familiar with the returning type used down the coast at King's Sound, but the non-returning type of the Northern Territory, occasionally brought in by visitors, was regarded as an object of mystery.[21] The pearl shells incised by the Karadjeri are associated with initiation, but the few which reached faraway South Australia were used for magic. The slate specimen at Ooldea was given a similar function.

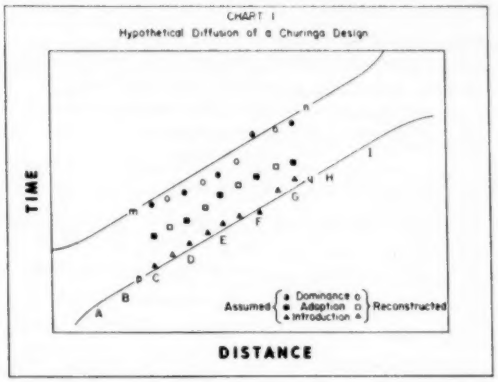
The time required to trade a specimen to a distant locality is unknown. Well organized trade routes have been shown by McCarthy to traverse the continent, but data are lacking on the frequency of exchange and the percentage of traded specimens which arrive at distant points compared with the number retained along the way. However, it should be noted that in desert areas such as eastern Western Australia a horde of about thirty-five persons may occupy 2,500 or more square miles. Thus, if an object passed in interhorde once a year it might move five hundred miles in a decade, a much greater distance if meetings were more frequent or exchanges were made between hordes assembled from afar. Some hordes travel two hundred miles or more to attend ceremonies. That an object ever traveled a great distance at maximum possible speed is to be doubted, but without specific evidence we cannot know, for example, whether the normal time for a decorated Karadjeri pearl shell to reach northern South Australia was five, ten, fifteen, or more years.

Churingas collected far from the historic area of manufacture are quite clearly strays, but so too are specimens found in the past at points within or near the present distribution of production but which were far from the area of manufacture at the time they were collected. For instance, if a churinga with the concentric circles motif had been collected at La Grange in 1875 it would not be evidence that the design had been applied there at that time. In other areas this question may be difficult to decide, but it should be anticipated in all localities on the present or past peripheries of any expanding distribution. With present information we could not be certain whether a similar specimen collected along the lower Fitzroy in 1875 had been made there or imported from the arid country to the southeast.

In contrast to random trade to distant places is the intentional introduction of churingas to a neighboring area over a period of a few or several years preliminary to the granting of permission to perform a ceremony and apply the associated design. During the interval, knowledge of the significance of the specimens and the ceremonies they represent presumably is acquired during intertribal visits. Thus the circumstances leading to the adoption of a design and the prominence subsequently attached to it are dictated by ceremonial context rather than artistic consideration. Just what happens to old ceremonies when a new ritual is acquired is uncertain. Some may be retained with diminishing importance, others modified, some abandoned. But in so far as the motifs are concerned it seems clear, at least in northeastern Western Australia, that the old patterns tend to become obsolescent as the result of the adoption of a new design. In recent generations the concentric circles and key motifs have been dynamic. In the second half of the nineteenth century, if not earlier, the key design spread eastward, southward, and westward from the Karadjeri into areas where the concentric circles motif was then lacking. It is not certain whether the southeastward movement was subsequently retarded if not stopped when it encountered the concentric circles design spreading from the east, but it is clear that the latter has become dominant in virtually all places where the two have met. The main indication that the eastern expansion of the key motif still retained some momentum in 1939 was the enthusiasm with which introduced specimens continued to be received by the Djaru. It should be noted that the Djaru applied the concentric circles motif at that time, but they had not yet given it dominant status. It would be important to learn to what extent the sweeping success of the concentric circles motif in so many areas has been due to its ceremonial context rather than to the fact that both ceremony and design have been the most recent to arrive.

The spread of a churinga design in northeastern Western Australia, as distinguished from the random trade of churingas, thus seems to be characterized by a sequence of three theoretically datable events—(1) introduction (without permission to reproduce), (2) adoption (following authoriza-

[21] Love (1936: 78) reports that one owner kept a specimen wrapped in paperbark and hidden from view. See also footnotes 6 and 7 in respect to curios.



tion), and (3) attainment of dominant status. The sequence is the same in each area, but the intervals between these events may vary widely. In one place permission to adopt may be given soon after introduction, in another only after many years. In one a dominant position may be gained relatively rapidly, in another conservative forces may favor retention of the older ceremonies for a much longer time. Nevertheless, since the three events are but different aspects of the same movement they are interrelated, and the variations in the intervals in each area and from area to area are relatively unimportant when seen in perspective of time and space. In theory the diffusion of a churinga design would seem to conform to the hypothetical case in chart 1. The events of introduction, adoption, and dominance are approximately parallel across the chart, yet in individual areas the intervals are quite variable. The general trend can be indicated by the lines *m-n* and *p-q* which define the greatest apparent range of the three events in any location. Whether this range spans a generation or many decades must be determined in each case. In theory the dates at all places should fall within the range indicated. If subsequently it should be found that some dates are not enclosed, as at *F*, the estimated range should be altered, or some local explanation sought. If the data for a specific case were as clear-cut as in the chart, a few deviations would not change appreciably the general trend suggested by the larger situation. The general trend also can be projected into the near past (*A, B*), and the immediate future (*H, I*), but allowances should be made for the possibility of major differences in range and pace the more remote the period.

It is difficult to estimate approximate dates for the diffusion of the concentric circles and key motifs. The type of event which signifies diffusion can seldom be assigned to a specific year even when data for several consecutive years are available. By what criteria does one decide that a design became dominant this year rather than last? Is it always clear to the aborigines that the specimens received last year were random trade pieces, but that those which arrived this year were intended to initiate influences which may not be comprehended for some time? Such changes presumably are seldom recognized at the time, yet in retrospect it may be common knowledge that a design, now dominant, was of minor importance several years previously, and unknown at a still earlier time.

The estimated dates are arranged by locality in charts 2–4. Whenever possible the approximations have been made to coincide with quarter-century years. For example, the concentric circles motif was a major design at La Grange in 1930 and it is assumed that it attained this status *circa* 1925. The date of adoption seems to have been a few years after the beginning of the century but is given as *ca.* 1900. In this case it seems certain that the interval between adoption and dominance was less than a quarter-century. There also are various instances in which the key motif is confined to similar maximum intervals between introduction and adoption on the one hand, and adoption and dominance on the other. In other cases the length of each interval is uncertain, al-

CHART 2

WESTWARD SPREAD OF CONCENTRIC CIRCLES MOTIF

	Warrawagine	←100 miles→	La Grange	←75 miles→	Eastern neighbors of Karadjeri
Dominant	—		*ca.* 1925	←20–25 years→	(Early 20th C.)
	↑		↑		↑
Adopted	(Not by 1939)	←	Early 20th C.	←	(Late 19th C.)
	↑		↑		↑
Introduced	Before 1930	←25–30 years→	Before 1900	←	(2nd half 19th C.)

CHART 3

WESTWARD AND EASTWARD SPREAD OF KEY MOTIF

	Warrawagine	←100 miles→	Karadjeri	←300 miles→		Djaru
				←75 miles→	Eastern neighbors of Karadjeri	
Dominant	*ca.* 1925	←	Sole possessors until about 1850?	→	Before 1900	—
Adopted	*ca.* 1900	←		→	Late 19th C.	(Not by 1939)
Introduced	Before 1900	←25–50 years→		→	*ca.* 1850?	*ca.* 1925
				←75 years→		

though the span of the two appears to be contained in a half-century or so.

The data are too few to establish the arbitrary quarter-century intervals and half-century spans as characteristic of the spread of both the key and concentric circles motifs, but they are suggestive and provide a tentative working basis. If they could be applied elsewhere we could assume, for example, that the concentric circles motif had been introduced in the eastern Canning Route area before 1847, or at least fifty years prior to 1897 at which time it already may have been dominant for some time.

The charts also throw light on the comparative dates of introduction, adoption, and dominance, respectively, from place to place. For example, the concentric circles design apparently was introduced at La Grange before 1900, at Warrawagine, one hundred miles to the west, before 1930. A thirty-three year time difference would indicate a rate of spread of three hundred miles per century, twenty-five or fifty year intervals, rates of four hundred or two hundred miles per century, respectively. There can be little doubt that this design was dominant just east of the Karadjeri at the beginning of, or early in, the twentieth century, and that it attained this status at La Grange, seventy-five miles to the west, *ca.* 1925. The twenty to twenty-five year interval indicates that attainment of dominance was moving westward at the rate of between 300 and 375 miles per century.

The key motif was introduced to eastern neighbors by the Karadjeri presumably not later than *ca.* 1850. If specimens first arrived in quantity among the Djaru, three hundred miles to the east, *ca.* 1925, the rate of spread was about four hundred miles per century, faster if the specimens were received at an earlier time, slower if the eastward movement began in 1825 or before. If churingas also were sent southward and westward *ca.* 1850, their arrival *ca.* 1900 at Warrawagine and Nullagine, one hundred and two hundred miles away, respectively, reflects a century rate of over two hundred miles in the one area and over four hundred miles in the other.

At what actual rates the designs were introduced, adopted, and given dominance in the various areas before 1900 is unknown. Without evidence from other places and other periods, as well as for other traits, we cannot know whether the estimated rates are unusually fast or slow, or attributable to local conditions, peculiarities of ceremonial context, or the recent change in the tempo

CHART 4

SOUTHWESTWARD SPREAD OF KEY MOTIF

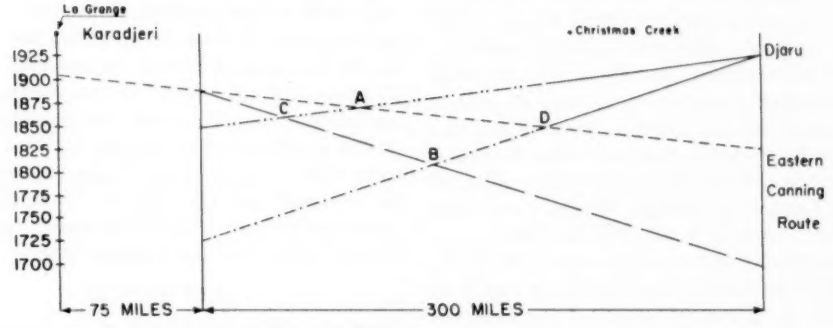
	Targudi	←100 miles→	Southern neighbors of Karadjeri	←100 miles→	Karadjeri
Dominant	*ca.* 1925	←	(*ca.* 1900)	←	Sole possessors until about 1850?
Adopted	*ca.* 1900	←	(*ca.* 1875?)	←	
Introduced	Late 19th C.	←	After 1850?	←	
		←50 years→			

of aboriginal life resulting from European influences. Furthermore, it now is too late to determine approximate dates for the nineteenth century. However, unless aboriginal culture disintegrates completely within the next few years, additional dates may be obtainable in the future. It would not be surprising to learn at any time that the concentric circles motif has been adopted as far west as the upper De Grey area, or north of the Kimberley ranges, the key motif north of the western Kimberley ranges or west of Lake Disappointment. The present situation at Warrawagine, Nullagine, Jigalong Creek, and other camps along the fringe of the desert is uncertain, but in 1939 conservative forces were sufficiently strong to have accepted new ceremonies and designs from the east if the time for such opportunities had been at hand. Further to the west and southwest the situation was obscure in 1939, for it was not clear whether conditions were still conducive to a further expansion of the key motif in these directions. However, it should be noted that remnant groups further west still talked in the present tense of various cultural movements, some of which possibly existed principally in memory. For example, old men at the Police Camp on the Fortesque River on the western periphery of circumcision asserted that their up-river neighbors were attempting to persuade them to adopt subincision. The few aborigines at Carnarvon eagerly anticipated the ultimate arrival of some new ceremony which they had heard had reached Mount Augustus on the upper Lyons River, two hundred miles to the east.

But to return to the question of the antiquity of the overlapping distributions of the decorative elements associated with slate churingas, we have seen that both the concentric circles and key motifs appear to have spread in some areas at the rate of about four hundred miles per century. If this rate prevailed during the second half of the nineteenth century the two distributions would have converged *ca.* 1875 at point *A* on chart 5. Such a date may seem rather late but it is not denied by

CHART 5

Place and Time of Convergence of Key and Concentric Circles Motifs at Different Hypothetical Rates of Spread



—·—·— Eastward spread of key motif at rate of 400 miles per century, and —·— at 150 miles per century from Karadjeri to undetermined point west of Christmas Creek.

——— Eastward trade of Churingas with key motif from undetermined point west of Christmas Creek to Djaru.

– – – – Westward spread of concentric circles motif at rate of 400 miles per century from Eastern Canning Route to La Grange and —— —— at rate of 150 miles per century from Eastern Canning Route to the Karadjeri frontier.

other considerations. On the other hand, if we assume that the rate of spread of both designs was only one hundred fifty miles per century, or slower than any estimated rate, convergence would have occurred at *B* only about sixty years earlier, or in the first quarter of the nineteenth century. At the four-hundred-mile rate the concentric circles motif would have passed the eastern Canning Route area in 1825 to arrive on the Karadjeri frontier about 1900, and duplicates of the churingas which left the Karadjeri around 1850 would have reached the Djaru *ca.* 1925. To reach the same places at the one hundred fifty mile rate, the concentric circles motif would have passed the eastern Canning Route area about 1700, and the key motif would have started eastward around 1725. If one motif spread at the faster rate, the other at the slower, the date of convergence would have been around the middle of the nineteenth century, the place of convergence further east or west (*C* or *D*) depending on which design traveled the faster.

It is easy to see from the chart how rapidly an area of overlap expands—on the average two hundred miles every quarter-century at the rate of four hundred miles per century, seventy-five miles every twenty-five years at the one hundred fifty mile per century rate, intermediate distances at slower or mixed rates. Thus, the greater the antiquity of slate specimens, the smaller the area in which the key and concentric circles designs could have been applied. However, it is not necessary to project the antiquity of these objects far back into the nineteenth century to explain all aspects of the situation as now known. If we grant that curios may be traded a thousand miles in fifteen, twenty-five, or forty years, the specimens collected at distant Ooldea and Nagga Wagga could have been made as recently as *ca.* 1900, *ca.* 1890, or *ca.* 1875, respectively, and convergence of the distributions of the key and concentric circles motifs as late as 1875, or at rate of spread of four hundred miles per century during the second half of the nineteenth century, would not be inconsistent.

The maximum possible antiquity of slate churingas cannot yet be given an approximate date, for it depends in part on the time when the distributions of the key and concentric circles motifs converged. Specimens with these designs could not have been decorated before these motifs spread into the area of manufacture, but all specimens with the zigzag and concentric rhombs motifs could have been made either prior to, or subsequent to, the first appearances of the former designs in the area, or some before and some after these arrivals. It is to be presumed that many more slate churingas remain to be collected, although it appears that they will not be abundant. The present scarcity warrants the suspicion that these objects were not made over a long period of time, for a tradition of manufacture implies continuity, with at least a few specimens produced each decade, if not each year. On the other hand, a much larger number would not necessarily imply a long period of manufacture, particularly if these objects were produced by several neighboring hordes. Many specimens could have been made in a few years even in a single horde if cultural circumstances demanded or enthusiasm for making the new type of churinga prevailed. Whether the interest in their manufacture lasted less than a generation or continued over the course of several decades or longer, or was confined to a few hordes, or to a single horde, is not yet in evidence. However, since the period of manufacture either straddled or followed the convergence of the distributions of the key and concentric circles motifs, the determination of this date would permit the working hypothesis that the maximum possible antiquity of slate churingas was not more than a few decades earlier. When evidence on the rate or rates of spread of the concentric circles and other motifs is available from other parts of eastern Western Australia and elsewhere in the continent it should be possible to see the situation in northeastern Western Australia in better perspective. However, the answers to most of the questions which pertain to slate churingas must come from the area of manufacture and it is to be hoped that they will be forthcoming when investigations are made in the region between the Fitzroy River and the Canning Route.

REFERENCES

BATES, D. 1938. The passing of the aborigines. London, Murray.

CARNEGIE, D. W. 1898. Spinifex and sand. London.

DAVIDSON, D. S. 1937. A preliminary consideration of aboriginal Australian decorative art. *Memoirs Amer. Philos. Soc.* **9**.

——. 1949. The Interlocking Key design in aboriginal Australian decorative art. *Mankind* **4** (3) : 85–98.

——. 1951. The Thread Cross in Australia. *Mankind* **4** (7) : 263–273.

ELKIN, A. P. 1932. Social organization in the Kimberley Division, north-western Australia. *Oceania* **2** (3) : 296–333.

——. 1936. Initiation in the Bard Tribe, North-west Australia. *Jour. and Proc. Roy. Soc. New South Wales* **69**: 190–208.

——. 1938. The Australian aborigines. Sydney, Angus and Robertson.

HAMMOND, J. E. (1913). Winjan's people. Perth, Imperial.

KLAATSCH, H. 1907. Schlussbericht über seine reise nach Australian. *Ztschr. f. Ethnol.* **39**: 635–690.

LOVE, J. R. B. 1936. Stone-age bushmen of to-day. London, Blackie.

MCCARTHY, F. D. 1939. "Trade" in aboriginal Australia, and "Trade" relationships with Torres Strait, New Guinea and Malaya. *Oceania* **9** (4), **10** (1–2).

PIDDINGTON, R. 1931. Karadjeri initiation. *Oceania* **3** (1) : 46–87.

PORTEUS, S. D. 1931. The psychology of a primitive people. N. Y., Longmans, Green.

SPENCER, B., and F. J. GILLEN. 1899. The native tribes of Central Australia. London, Macmillan.

THORP, W. W. 1923. An aboriginal magic plate. *Records, Australian Museum* **14** (1) : 60–61.

THE CULTURES OF EARLY ANATOLIA

ALBRECHT GOETZE

William M. Laffan Professor of Assyriology and Babylonian Literature, Yale University

(*Read November 14, 1952*)

PREHISTORY is anonymous; history always deals with individual personalities or at least with individual peoples. The historian who has followed up to the beginning of written records the particular civilization with which he is concerned will hesitate before he takes the step over the threshold into the semi-darkness of anonymity. If he is convinced, as I am, that he must make use of all the sources at his disposal, archeological, linguistic, and philological, he will nevertheless take it. He will parallel a controllable historical situation with an archeological and a linguistic situation and working back from such a basis try to light up at least to some extent the prehistoric past. I propose here* to make an attempt of this kind for Anatolia.[1]

The second millennium of the peninsula has been brought into the full light of history by two great complexes of clay tablets inscribed in cuneiform, the Hittite tablets from Boğazköy, ancient Ḫattušaš, and the so-called "Cappadocian" tablets which in their majority come from a site near Kültepe, ancient Kaniš. The Hittite tablets illuminate not only the period of the great Hittite Empire of the centuries from 1400 to 1200 B.C. but lead us back to about 1900 B.C.[2] The "Cappadocian" or Kültepe tablets—linguistically Old Assyrian—bear witness of the activities of Assyrian merchants in Anatolia just dovetailing with the beginnings of Hittite history and in general antedating it;[3] they are contemporaneous with the reigns of king Šarrum-kēn of Assur and both his immediate predecessor

* In this article the following abbreviations of more frequently quoted publications will be used:

Bittel, PFK. Prähistorische Forschung in Kleinasien (Istanbul, Arch. Inst. des Deutschen Reiches, 1934).

Bittel, KS. Kleinasiatische Studien (Istanbul, Arch. Inst. des Deutschen Reiches, 1942).

Bittel, Gz. Grundzüge der Vor- und Frühgeschichte Kleinasiens (Tübingen, E. Wasmuth, 1st ed. 1945, 2nd ed. 1950).

Götze, Kleinasien. Kulturgeschichte des Alten Orients (im Handbuch der Altertumswissenschaft) III/1 (München, C. H. Beck, 1933).

Periodicals, serials, etc. are quoted according to the following system:

AAA. Annals of Archaeology and Anthropology
ABAW. Abhandlungen der Bayerischen Akademie der Wissenschaften
AfO. Archiv für Orientforschung
AJA. American Journal of Archaeology
APAW. Abhandlungen der Preussischen Akademie der Wissenschaften
Arch. Or. Archiv Orientální
BASOR. Bulletin of the American Schools of Oriental Research
Bell. (Türk Tarih Kurumu) Belleten
BSA. Annual of the British School at Athens
ILN. Illustrated London News
JAOS. Journal of the American Oriental Society
JCS. Journal of Cuneiform Studies
JKF. Jahrbuch für Kleinasiatische Forschung
KUB. Keilschrifturkunden aus Boghazköi
MDOG. Mitteilungen der Deutschen Orient-Gesellschaft
OIP. Oriental Institute Publications
OLZ. Orientalische Literaturzeitung
Orient. Orientalia
PZ. Prähistorische Zeitschrift
RA. Revue d'Assyriologie
RF. Reinecke-Festschrift (Mainz, 1950)
RHA. Revue Hittite et Asianique
Stud. Orient. Studia Orientalia
WVDOG. Wissenschaftliche Veröffentlichung der Deutschen Orient-Gesellschaft
ZA. Zeitschrift für Assyriologie
ZDMG. Zeitschrift der Deutschen Morgenländischen Gesellschaft

[1] For similar attempts see W. Lamb, Anatolian Studies pres. to W. H. Buckler, 129–150, 1939; K. Bittel, RF, 13–25, 1950, and *Historia* 1: 267–286, 1950.

[2] For a comprehensive survey of all questions connected with early Anatolia see A. Götze, Kleinasien. More recent books are, e.g., L. Delaporte, *Les Hittites*, Paris, La Renaissance du Livre, 1936; G. Contenau, *La Civilisation des Hittites*, ed. revue et augmentée, Paris, Payot, 1948; E. Cavaignac, *Les Hittites*, Paris, A. Maisonneuve, 1950; they do not contribute anything essentially new.

[3] Besides the survey quoted in footnote 2 see the introduction to I. J. Gelb, *Inscriptions from Alishar and vicinity* (OIP **27**, Chicago, 1935). The edition of texts has been continued since 1933 and the excavations conducted by the Türk Tarih Kurumu at the "Kārum Kaniš" (see below fn. 21) has brought to light much new material.

and successor and cover about the twentieth century B.C.[4]

In the following I shall *first* outline the historical and linguistic situation which we can reconstruct from these records, *then* proceed to a brief evaluation of the archeological findings which parallel them and guide us beyond them, and *finally* attempt a synthesis between the results obtained by the examination of the documents on the one hand, and of the monuments on the other.

I

In such an endeavor we are helped by various factors. There is in the first place the remarkable historical sense of the Hittites who have left records that lead back to the very beginnings of their national history. In the second place we benefit from their extraordinary religious conservatism which resulted in the custom of continuing indefinitely the cults of ancient deities in the language of the communities whence they had been taken over. Furthermore we are aided by the analysis of the numerous proper names contained in the two large bodies of source material.

The people whom we call Hittites say of themselves that they speak ***naš-ili*** "in the manner of (the city) Nešaš" or ***nešumn-ili*** "in the manner of Nešites." (The location of Nešaš has not been identified with exactness, but it must be somewhere near the southern edge of the Anatolian plateau.) This is a language which reveals itself as related to the Indo-European languages.[5] The Hittites, at the peak of their power which falls in the centuries between 1400 and 1200, created an empire that became the mightiest in the world; their civilization developed to heights which placed them as equals at the side of their contemporaries in Egypt, Assyria, and Babylonia. The kingdom of Ḫattušaš succeeding that of Kuššar arose from the rivalry between a number of smaller kingdoms—among them that of Nešaš—in which the weaker ones were eliminated by the stronger. The text of Anittaš of Kuššar composed in Hittite[6] describes it in some detail. By a fortu-

[4] Šarrum-kēn (Sargon) of Aššur is the great-grandfather of Erišum II who was deposed by Šamši-Adad I. As the Mari texts have revealed the latter is an older contemporary of Ḫammurapi of Babylon. Šarrum-kēn must, therefore, be placed about a century before Ḫammurapi. As to that king, I follow a chronology which is *ca.* 60 years higher than that of S. Smith and *ca.* 100 years higher than that of W. F. Albright; see Meeting of the American Oriental Society 1950 Abstracts of Communications sub No. 23 (*cf. Orient.* NS **19**: 378, 1950); BASOR **122**: 18 ff., April 1951; JAOS **72**: 67 ff., 1952; BASOR **127**: 21 ff., Oct. 1952. Bittel (RF, 1950) follows the chronology of Cornelius (which coincides with that of Albright) as though it were proved and generally adopted.

[5] It is immaterial in our present context whether Hittite is just one other Indoeuropean language—this is the opinion of a majority of scholars including H. Pedersen and F. Sommer—or a sister language of the reconstructed Indoeuropean language from which the others descend—this is the opinion first advanced by E. Forrer and elaborated on by E. H. Sturtevant.

[6] The problem as to the authenticity of this inscription has been much discussed, see K. Bittel, Boğazköy (APAW, 1935, phil.-hist. Klasse Nr. 1) 13; K. Bittel, Kleinfunde (60. WVDOG, Leipzig, J. C. Hinrichs, 1937) 61 f. Landsberger still maintains—see *Arch. Or.* **18**1/**2**: 343 fn. 69, 1950, —that it is apocryphal, a point of view which, under Landsberger's influence, Güterbock had set forth in detail (ZA NF 10, 1938, 139 ff.). As myself (Haverford Symposium 1938, 146 f.) both Bittel and Otten now believe that the text is based on a trustworthy old source and that the events related therein actually took place in substantially the narrated way (see K. Bittel, Gz., 34 f., 1945; *Historia* **1**: 270, 1950, and H. Otten, MDOG **83**: 38 ff., 1951). Compare furthermore H. Th. Bossert, Königssiegel (Berlin, Arch. Inst. des Deutschen Reiches, 1944) 21 and S. Alp, JKF **1**: 125 f., 1950.

Landsberger's judgment of the situation rests on several presuppositions: (1) that Anitta is a Ḫattian, (2) that before the Mari Age the Anatolian languages were not written at all, or not written in cuneiform. I cannot agree with either supposition. As to (1), Landsberger recognizes at the time of the Cappadocian tablets only (Proto-)Ḫattians and Proto-Luwians (who are said to be non-Indoeuropeans). In doing so he is compelled to remove from Hittite as foreign accretions all those lexical elements which are common to Hittite and the "Asianic" proper names of the Cappadocian tablets, moreover all the suffixal elements like *-uman, -aḫšu, -šar* found in proper names. The treatment of E. Laroche in his Recueil d'onomastique hittite (Paris, C. Klincksieck, 1952) leads to quite different results which—from the point of view of Hittitology—seem better founded. If the occurrence of *Ḫattuša* (with final *-a*) instead of Ḫattian *Ḫattuš* in Mari proves that "Nešite" had become current (*Arch. Or.* **18**1/**2**: fn. 67 sub No. 4), should not the appearance of *Kuššara* beside *Kuššar* already in Kültepe tablets (*cf.* Bilgiç, AfO **15**: 34, 1951) force us to predate that shift into the period of the Old Assyrian tablets? The conclusions of Alp (JKF **1**: 125, 1950) based on Kültepe names in *-ala, -ili, -ula* etc. which continue ḫattic formations in *-l* are also pertinent.

The alleged illiteracy of the "Anatolians"—point (2)—is based on an *argumentum e silentio* which, in fact, is inadmissible. Personally I would not be astonished at all were Hittite tablets discovered in Anitta or pre-Anitta layers. The Hittite script, as is now well known, dates back to the period of Ur III at the latest. Are its antecedents just Hittite or are they Syrian? The Syrians of the Mari Age seem to have written a type of script more closely related to Old Babylonian. At any rate, one must not measure script and orthography of the Anitta texts on Old Babylonian usage (*Arch. Or.* **18**1/**2**: 342 fn. 68);

nate accident this same king and his father Pitḫanaš also occur as native rulers in the Kültepe texts.[7] There is no valid reason for doubting that the Hittite element was already present in Anatolia in the twentieth century B.C. The proper names on Kültepe tablets support that contention.[8]

They also prove the presence of a particularly strong Ḫattic stratum in the population. These are the people who—to use the term of the Boğazköy texts—speak *ḫattili* "in the manner of Ḫattians."[9] Their language survives in the cult of many gods, among them the most important ones of Asia Minor, e.g., the Stormgod Taru worshiped in the image of a bull, the deity Inar worshiped as a stag, the Sungod Ištanu, and—at the head of the whole pantheon—the Sungoddess Wurušemu.

To the Hittites and the Ḫattians as components in the population of Anatolia around 2000 B.C. the Luwians must be added, who, at times, seem to have been even more important than the Hittites. There are gods who must be addressed *luwili*, and elements of this language, or dialects related to it,[10] likewise emerge in the onomasticon of early Cappadocia. Luwian is another language of Indo-European habitus and related to Hittite. Since Luwiya, the corresponding "land" must be localized along the southern shore of Asia Minor and particularly in its western part, we may well speak of an influx of southwestern elements onto the highlands of Anatolia.[11]

II

Let me now turn to the archeological situation as it has been revealed on an ever increasing scale by excavations. We shall briefly examine the stratification which has been observed at a few key sites.

The first fact which is forced upon us is the dichotomy between East and West.[12] It is quite naturally the consequence of geographic factors. An arid region in the center of Anatolia must be skirted by routes that follow the mountain ranges of the north and of the south. When only Troy in the west and Alişar in the east were excavated, this dichotomy seemed more incisive than it appears now when intermediate sites have become known where Eastern and Western elements mix.[13] However, it remains a basic fact in early Anatolian archeology.

The Eastern type site is still Alişar.[14] There the following successive cultures—each characterized in many ways and especially by the ever-present pottery—have been observed: on the top of the tell

the basis of Hittite script and orthography is considerably older than the Old Babylonian period which, in many respects, is full of revolutionizing innovations.

[7] J. Lewy, RHA **17**: 1 ff., 1934.

[8] The problem as to the appurtenance of the proper names in *-uman*, *-umna* to Hittite or Luwian which Bittel, Kleinfunde (60. WVDOG, Leipzig, J. C. Hinrichs, 1937) 62—following Gelb (OIP **27**: 14 ff., Chicago, 1935), and Sommer (OLZ, 278, 1935)—had declared as solved in the negative sense is after almost twenty years still very much alive. See most recently E. Laroche, Recueil d'onomastique hittite (Paris, C. Klincksieck, 1952) 104, who attributes them to the language of Kaniš which he assumes was a Luwian dialect.

[9] For this language see recently Laroche, RA **41**: 67 ff., 1947; JCS **1**: 187 ff., 1947; JKF **1**: 174 ff., 1950/1. The Ḫattic influence is particularly noticeable in the field of religion; on this point see E. Laroche, *Recherches sur les noms des dieux hittites* (Paris, G. P. Maisonneuve, 1947) and H. G. Güterbock, Hittite Religion in V. Ferm, *Forgotten Religions*, 83 ff., N. Y., Philosophical Library, 1949.

[10] Additional material for Luwian will be presented in KUB XXXV (in press); Luwian is related to the language of the hieroglyphic inscription on the one hand and the one represented by the elements marked by the "Glossenkeil" on the other.—The term "Proto-Luwian" for a (hypothetic) language from which part of the names of the natives in the Kültepe tablets is taken (Bilgiç, AfO **15**: 15, 1951) and which is said to be Pre-Indoeuropean is not very fortunate. It is bad enough that we have to wrestle with Hittite and Proto-Hittite (which is Ḫattic!). For what reasons Landsberger (Sam'al, Ankara, Türk. Hist. Gesellschaft, 1948, 102 ff.) posited five Luwian dialects ("Luisch I, II, III, IV, V") is not clear as long as the chapter B to which he refers remains unpublished. We have so far operated with the three mentioned varieties.

[11] Ḫatti, Luwiya and Palā are in older Hittite texts mentioned as the three main Anatolian areas within their horizon. Since the border between Ḫatti and Luwiya lies south of Tuwanuwa (Tyana) and Uda (not identified with certainty as yet), and since furthermore the plain of Cilicia must be regarded as part of Kizzuwatna, it follows that Luwiya must lie further toward the west.—We have recently learned (Otten, ZA NF **14**, 1945) that Palaic—some people speak *palaumnili*—is related to Hittite and Luwian, in other words the three mentioned countries are (the) three in which "Indoeuropeans" must have settled.

[12] A. Götze, Kleinasien, 24 ff., 1933; K. Bittel, PFK, 10 ff., 1934; Gz., 8 ff., 1945.

[13] Bittel in the last named book, and before in KS, 127 ff., 1942, stressed the marginal groups that mediate between the two main cultural provinces.

[14] The results of the excavations conducted 1927–1932 on behalf of the Oriental Institute of Chicago by H. H. von der Osten, Erich F. Schmidt and others are published in seven large volumes OIP **6**, **7**, **19**, **20**, **28–30** (Chicago, 1930–37). For a survey of the results see especially K. Bittel, PFK, 12 ff., 1934; Gz., 15 ff., 1945.

"Phrygian": Alişar IV,
Hittite: Alişar II

and in its lower reaches layers of an

Early Bronze Age:[15] Alişar I.

Even the earliest of these civilizations is an urban civilization. Below it remnants of a village have appeared which mark the

Chalcolothic Age: Alişar 0.

The upper and the lower strata are separated by a relatively insignificant layer (in the beginning mistakenly numbered Alişar III) carrying a painted and handmade pottery, known as "Cappadocian" Ware, which contrasts with monochrome pottery above and below.

Alişar has yielded some Cappadocian tablets and their position in the sequence is significant not only for cultural history, but also for ascertaining an absolute chronology: they come from level 10c which is the lowest sublevel of the second last level within II.[16] Here, then, do we get our last absolute date—1950 B.C.—and learn at the same time that the culture which to archeologists is known as "Hittite" was already in existence when the Assyrian merchants entered Anatolia. Moreover it proves to grow—a younger development—out of I.[17]

It may be remarked already here that in the final phase of I examples of the so-called "depas" appear[18] (a characteristic pottery shape which will be mentioned again presently).

The relatively early beginning of the "Hittite" civilization is borne out by Boğazköy and in particular by Kültepe.

In Boğazköy[19] we obtain below an extensive "Phrygian" layer (Boğazköy II) two Hittite layers (III and IV) representing the younger and the older kingdoms respectively—i.e., in terms of years 1200–1400 and 1400–1900. "Cappadocian" tablets appear here in V, the deepest layer reached in stratified areas. The so-called "House on the slope"—without stratigraphical connections—leads back into the same period as Alişar I. Cappadocian pottery has emerged only in a few isolated instances.[20]

The recent work of Turkish excavators at the center of the Assyrian merchants near Kaniš—at the so-called *kārum Kaniš*—[21] is particularly significant because it shows the tablet carrying layer of the twentieth century (Kültepe II[22]) sandwiched between a younger "Hittite layer" (Kültepe I) and two older layers of which III is architecturally closely connected with II and IV brings the bulk of the Cappadocian pottery which at this side is very abundant. It becomes evident here that the flourishing period of that significant ware antedates the twentieth century not inconsiderably.[23]

At this point a few remarks may be inserted on Alaca Hüyük[24] not because it adds anything new stratigraphically, but because it contributes in a startling fashion to our knowledge of the Early Bronze Age which at Alaca ends in an apparent catastrophe. The excavators were lucky enough to hit upon the tombs of local princes, full of precious objects made of gold, silver, electron, iron, amber etc., and probably contemporaneous with the Akkad period of Mesopotamia.[25] For the purposes of this paper I

[15] Turkish archeologists use the term "Copper Age" instead. Our term "Early Bronze Age" had, mistakenly, been applied to Alişar II (see presently) by the excavators.

[16] *Cf.* JAOS **59**: 512, 1939.

[17] This view was independently expressed by Bittel (PFK, 67, 72, 1934) and myself (Kleinasien, 39, 1933) and we have found the support of W. Lamb, Anat. Stud. presented to W. H. Buckler (1939) 135 f. But von der Osten (OIP **30**: 444 ff., 1937) takes the opposite view.

[18] H. H. von der Osten, OIP **30**: 420, 1937; K. Bittel, KS, 132, 1942.

[19] See Bittel's annual reports in MDOG **70–78**; furthermore K. Bittel and H. G. Güterbock, Boğazköy (APAW 1935 phil.-hist. Klasse Nr. 1); K. Bittel and R. Naumann, Boğazköy II (APAW 1938 phil.-hist. Klasse Nr. 1).

[20] See K. Bittel, Gz., 38, 1945; but note that the layers that fall between V and those represented by the "House on the Slope" have apparently not been struck as yet.

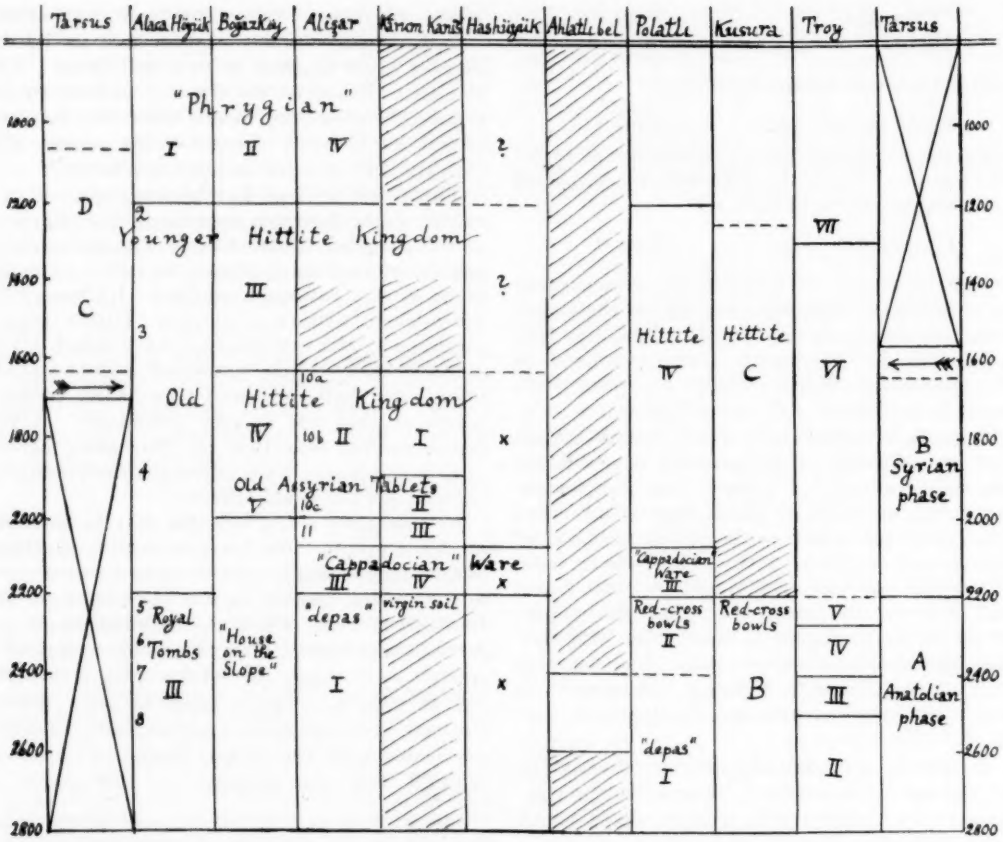
[21] T. Özgüç, Kültepe Kazisi Raporu (Türk Tarih Kurumu Yayinlarindan V/10, 1950). Furthermore T. and N. Özgüç, ILN **5722**, Dec. 18, 1948; **5778**, Jan. 14, 1950; **5868** (Oct. 6, 1951); see also AJA **54**: 61 ff., 1950; **55**: 91 ff., 1951; AfO **15**: 155 ff., 1945–1951.

[22] The only tablets found in III are those from the house of Adad-şullūlī. However, as K. Balkan pointed out at the Int. Congress of Orientalists in Istanbul 1951, these tablets are dated to the same *limmus* (archons) and sealed with the same seals as those from the house slightly sideways above it in II; they cannot be separated from them. This means that tablets occur not earlier than in II.

[23] Its peak falls into level IV; it is petering out in III.

[24] Remzi Oğuz Arik, Les fouilles d'Alaca Höyük 1935 (Publ. de la Soc. d'Hist. Turque V/1, Ankara, 1937); Hamit Zübeyr Koşay, Alaca Höyük Hafriyati 1936 (Türk Tarih Kurumu Yayinlarindan V/2, Ankara, 1938) (German edition, *ibid.* 2a, Ankara, 1944); the same, Alaca Höyük Kazisi 1937–39 (*ibid.* V/5, Ankara, 1951). Furthermore La Turquie Kemaliste **32–40**: 20–26, 1939/40; Koşay and Akok, AJA **51**: 152 ff., 1947. See also Bittel's annual reports in Archäologischer Anzeiger 1939 ff.

[25] After Hamit Zübeyr Koşay especially K. Bittel, RF, 20, 1950; *Historia* **1**: 273, 1950.



stress the "standards" which testify vividly to the worship of a bull god and of a stag god.[26]

The most important archeological sequence of western Asia Minor is presented by Troy for the older phases of which we now possess the results of Blegen's meticulous check,[27] which leads in exactness far beyond the findings of Schliemann and Dörpfeld.[28] I can be brief since Troy VI, the "Homeric" town, and the earlier Troy II with its remarkable treasures are widely known. The native pottery which II, and after its destruction III–V, have yielded are characteristically different in technique, shapes, and decor from anything the east of the peninsula has to offer.[29] The end of Troy VI falls somewhere around 1300 B.C.; the exact chronological position of the beginning of Troy VI[30] and of the catastrophe which overtook Troy II[31]—dates so

[26] *Passim* in the volumes quoted in fn. 24.

[27] C. W. Blegen (and others), Troy Vol. 1 (Princeton, Princeton Univ. Press, 1950), Vol. 2 (*ibid.*, 1951); this publication supersedes—as far as it goes at present—the preliminary reports which were published in AJA **36–43**, 1933–39.

[28] See W. Dörpfeld, Troja and Ilion (Athens, Beck and Barth, 1902); Hubert Schmidt, H. Schliemann's Sammlung trojanischer Altertümer (Berlin, G. Reimer, 1902).

[29] The literature can be easily found in K. Bittel, KS, 127 ff., 1942; Gz., 108 f.

[30] 1700 B.C.: F. Matz, Gnomon **16**: 152, 1940; *Handbuch der Archäologie* **2**: chart, München, Beck, 1950; 1750 B.C.: F. Schachermeyr, PZ **34/5**: 40, 1949/50; 1900 B.C.: K. Bittel, KS 131, 1942; Gz. chart (=Abb. 52), 1945. Bittel's date as well as Matz's is dependent on the assumption that the shift from EH to MH falls within Troy V. This position is no longer tenable. J. L. Caskey, who was on Blegen's staff, stated emphatically (AJA **52**: 119 [1948]) that no MH pottery occurs in Troy V, and Blegen himself, attributing all gray "Minyan" ware to Troy VI, parallels V with the final phase of the Early Bronze Age in the Aegean and on the Helladic mainland (Troy II/1 [1951] 229).

[31] 2200 B.C.: F. Matz, *Handbuch der Archäologie* **2**: chart, München, Beck, 1950; *Historia* **1**: 189, 193, 1950;

important for pre-historic chronology—have been much discussed and produced a great variety of opinions.

For a link-up between the east and the west of Anatolia the sounding conducted in 1949 at the hüyük of Polatli—on the railroad from Eskişehir to Ankara just east of the Sakarya River, i.e., midway between the two halves of the central plateau—is of great value. At this site four main periods could be distinguished and they are characterized, in stratified order, by the following wares or forms:

Hittite wares:	Polatli IV,
"Cappadocian" ware:	Polatli III,
Red-Cross bowls:	Polatli II,
"Depas":	Polatli I.[32]

The significance of this order becomes apparent if the fact is recalled that the Red-Cross bowl is "the hallmark par excellence" of the Fifth Trojan settlement[33] and occurs also in the last phases of Kusura B[34] and in Tarsus A[35] (the "Anatolian" period of that latter town).

The "depas," so familiar from the upper layers of Troy II and also from Troy III/IV, belongs in Polatli to the end of I. It recurs—probably in slightly younger forms—in Alişar I and Alaca III.[36]

It is immediately apparent that these circumstances militate against dates for the end of Troy V as late as 1700 or 1900, and dates for the violent destruction of Troy II as late as 2200. Even a date of 2300 for the latter event seems still too low. The Red-Cross bowls of Polatli II, certainly contemporaneous with Troy V, are separated by more than 6 meters of deposit from Polatli IV,[37] and the bottom of Polatli IV must be coördinated with Alişar II level 11 and Kültepe III, layers which are still below the strata that include Cappadocian tablets. The Red-Cross bowls, then, must have been produced not inconsiderably before the Cappadocian tablets. In the circumstances a date for them, and thereby also for Troy V, before 2200 B.C. seems on the conservative side. If we assign to Troy III–V a minimum of 300 years—this is less than the minimum which Blegen advocates[38]—it means that Troy II was destroyed around the middle of the third millennium.

It is not easy to say whether Polatli should be described as a member of the western or the eastern family. At any rate, the question must be answered for the various periods separately. I would label I and II as western, while IV is to an increasing extent easternized[39] without, however, losing a western flavor. In between falls III which is an intrusion at any event.

The situation in the southwest, in Kusura,[40] is similar. There B is western, closely related to Troy, and likewise contains Red-Cross bowls, but in C—which follows B after a violent destruction—Hittite, i.e., eastern, influence is dominant.

An important site is also Ahlatlibel[41] in the middle. The black burnished ware that at that place is mixed with Alişar I ware betrays in technique and shapes western affiliations;[42] its

2150 B.C.: F. Schachermeyr, PZ **34/35**: 40, 1949/50; "Um 2200 oder 2100 v. Chr.": K. Bittel, *Historia* **1**: 269, 1950; 2300: S. Weinberg, AJA **55**: 406, 1951; see furthermore V. Milojčic, BSA **44**: 304, 1949.

[32] S. Lloyd and Nuri Gökçe in *Anatolian Studies* **1**: 21 ff., British Inst. of Arch. at Ankara, 1951.—At first sight it may seem surprising for "Cappadocian" pottery to appear so far toward the west. Faced with S. Lloyd's substantial description and the endorsement which T. Özgüç seems to have given the identification, I see no possibility how the fact could be explained away. It must furthermore be remarked that the lack of substantial quantities of Cappadocian ware at Boğazköy is probably due to the fact that the respective layers have at best been scratched. In Hashüyük (about which, to be true, not even a preliminary report has been published) the occurrence must be more than "sporadic" (Bittel, Gz. Karte 4) if the statements made in MDOG **70**: 30 and PFK **15** are valid. The few sherds from Karaoğlan (*cf.* now also H. H. von der Osten, *Orient. Suecana* **1**: 21, 1952) come from a site west of the Halys.

[33] C. W. Blegen etc., Troy II/1 (1951) 250.

[34] W. Lamb, Excavations at Kusura near Afyon Karahisar (*Archaeologia* **86**, 1937) 11, 17.

[35] H. Goldman, AJA **44**: 65, 1940; **51**: 387, 1947. *Cf* for Mersin, AAA **26**: 132, 1940.

[36] On the "depas" and its significance for chronology see K. Bittel, PFK, 68, 1934; KS, 132 ff., 1942; RF, 22, 1950. There are close analogues in 'Amuq J (=Judaideh IX), the phase of the "chalciform pot series" (see H. Ingholt, Hama 49 fn. 1; also AJA **55**: 406).

[37] S. Lloyd's term "Kültepe Period" for Polatli IV is unfortunate and implies a date which in my opinion is mistaken. The tablets are in Kültepe itself separated from the "Cappadocian" pottery by most of the level III which according to the latest revision contains no tablets.

[38] Troy III: "not less than a century" (Troy II/1 10); Troy IV: "some 150 years" (*ibid.* 111); Troy V: "at least a century, perhaps 150 years" (*ibid.* 230).

[39] *Cf.* S. Lloyd and N. Gökçe, *Anatolian Studies* **1**: 59 f., 1951.

[40] W. Lamb, Excavations at Kusura near Afyon Karahisar, *Archaeologia* **86**: 1 ff., 1937; **87**: 217 ff., 1938.

[41] Hamit Zübeyr Koşay, *Türk Tarih Arkeologya ve Etnografya Dergisi* **2**: 3 ff., 1934; Bittel, PFK, 72 ff., 1934; AfO **11**: 38 ff., 1936.

[42] Hamit Zübeyr Koşay, l. c. 12; von der Osten, OIP **30**: 422, 1937; K. Bittel, KS, 183 ff., 1942; Gz., 16, 1945.

closest relatives are found in Pisidia.[43] Further toward the east it appears in Alaca as an admixture to, and side by side with, Alişar I pottery.[44]

So as to make clearer the points which are significant in our context it seems useful to summarize the sketched situation as follows:

Between *ca.* 3000 B.C. and 1200 B.C. we find in Asia Minor two main cultures, an eastern and a western one, touching each other in the middle.

The eastern one grows out of the chalcolithic period to develop organically into the Early Bronze Age and finally to blossom forth as a Hittite civilization. Between the Early Bronze Age and the Hittite period we observe an intrusion which is characterized by the Cappadocian ware.[45] The eastern culture expands with the Hittites into the west where its influence becomes strong both in Kusura and Polatli. During the last two thirds of the millennium it makes the contemporaneous civilization of Cilicia appear as essentially Hittite.

The western civilization had passed its peak at that time. In the third millennium it had included not only Cilicia but, over Ahlatlibel, had penetrated the east where it left traces in Alişar itself. It shrank in territory after *ca.* 2200 B.C.

In the main, then, there are three elements recognizable:(1) the East Anatolian component, (2) the West Anatolian component, (3) the intruding element which begins *ca.* 2200 and is eliminated within *ca.* 150 years.

III

The synthesis between the historical and the archeological evidence here to be advanced has inevitably been hinted at during the foregoing sketch.

The east Anatolian civilization of the second millennium, which presents itself to the historian in Hittite disguise, is deeply rooted in the earlier past of the peninsula. It traces back into the Early Bronze Age which certainly was the floruit of the Ḫattians. These appear to us, at our present stage of knowledge, as autochthonous with perhaps Caucasian relationships.

The coming of the Hittites—not necessarily very numerous—which altered not only the political structure of the land but also involved a linguistic change coincides with three archeological phenomena: (1) the layer of destruction which is observable in many sites[46]—among those mentioned here clearly in Alaca Hüyük in Polatli and in Kusura; (2) new, and right from the beginning elaborate, shapes of pottery afterward; (3) the appearance of the so-called "Cappadocian" ware. Whether we see in the bearers of the new painted ware the Indo-European element or not,[47] it seems obvious that the phenomenon as such is related in one way or another to the influx of a new component in the population. Civilization as a whole acquires thereafter a new appearance, one that is more vigorous than it had been before.[48]

The western component must in all probability be termed "Luwian." The western features of eastern sites like Ahlatlibel, Alaca, and even Kültepe and Alişar are best paralleled with the Luwian elements in the nomenclature of the Kültepe tablets. Related elements survived till Roman times in the Anatolian southwest.[49]

---

[43] K. Bittel, KS, 176 ff., 1942.

[44] Hamit Zübeyr Koşay, *Ausgrabungen von Alaca Höyük 1936*, 177, 1944. Western pots from Kültepe, unfortunately unstratified (Bittel, PFK 69 with plate i) show that western influence reached even that eastern stronghold.

[45] Distribution: Bittel, PFK, 94, 1934 with fn. 1; Gz., map 4, 1945.

[46] Particularly clear is the situation in Dündartepe (south of Samsun), see Bell. **35**: 316 ff., 1945; *Orient.* NS **16**: 264 ff., 1947. Places like Karaoğlan and Bitik were destroyed and never resettled.

[47] I had formerly maintained (Kleinasien, 46 ff., 1933) that the bearers of the "Cappadocian" ware were Hittites. I now prefer the more cautious attitude taken in the text. I still think that there is a connection, but it may be indirect (*cf.* K. Bittel, Gz. 39 who denies "direct connection"). The historical events are not known to us and they may have been much more complex than anyone can imagine at present.

[48] This point is justly stressed by K. Bittel, *Historia* **1**: 274 ff., 1950.—The question as to whether the new element penetrated Cappadocia from the west or from the east is of a quite secondary order. The historical records suggest that the "Hittites" established themselves first in the south of the plateau around Mons Argaeus before they reached Ḫattušaš further north. They may even have settled at some time further toward the southeast and have had contacts with Mesopotamia there. And yet, even that would not prove a northeastern route of immigration. It may simply mean that the newcomers backtracked on the way they had already left behind.

[49] Rich material is collected by J. Sundwall in Die einheimischen Namen der Lykier (*Klio, Beiheft* **11**, 1913); Kleinasiatische Nachträge (*Stud. Orient. edid. Soc. Orient Fennica* XVI/1, 1950). *Cf.* also E. Laroche, *Recueil d'onomastique hittite*, 144 ff., 1952.

All this suggests anew[50] that the Luwians played an important part in Anatolia as far back as the late third millennium. How important, can be seen from the wide distribution of the *ss*-suffix in the geographic nomenclature not only of Asia Minor but also of Greece. This suffix does occupy an important place in the morphology of Luwian and of the language written in Hittite hieroglyphs and it serves well for the explanation of the place names in question.[51] If a people speaking a related dialect could spread over the islands and eastern Greece in Early Helladic times[52] the appearance of Luwian elements in the east at about the same time is brought into sharper focus.

Beyond that point the third millennium is too far removed from the historical periods as to project profitably back into it historical nomenclature. It seems likely, however, that the Luwians were injected into the Anatolian picture at a relatively late moment during the Early Bronze Age. As a whole the Anatolian cultures of the third millennium in the west as well as in the east, different though they are from each other in many respects, nevertheless resemble each other in a general way[53] and are in all probability not only Pre-Hittite, but also Pre-Luwian.

[50] *Cf.* Kleinasien, 55, 1933. Perhaps I was too bold to ascribe the whole Troy-Yortan complex to the Luwians. Even in the new, more moderate form we can expect that the thesis will be opposed by all those who are afraid to admit the presence of "Indoeuropeans" in Asia Minor (and the Aegean world) by the end of the third millennium. In the circumstances it is quite gratifying to read in Bittel's Gz. (1st ed. 49, 2nd ed. 61) the sentence: "Götze's frühere Theorie hätte mit der Einschränkung Geltung, dass [für die Sitze der ersten Indogermanen] ein enger begrenztes Gebiet innerhalb der Troia-Yortankultur in Betracht käme." It may be remarked in passing that the followers of Kretschmer ("Proto-indogermanische Schicht") and of Sturtevant ("Indo-Hittite hypothesis") will feel less hesitation to follow me.

[51] E. Forrer, ZDMG NF 1: 218 ff., 1922; F. Sommer, Aḫḫijavāfrage und Sprachwissenschaft (ABAW, philos.-hist. Abt. NF 9, 1934) 84 ff.; E. Laroche, *Recherches sur les noms des dieux hittites*, 68 ff., 1947; Alp, JKF 1: 132, 1950.

[52] The article, The coming of the Greeks, by J. B. Haley and C. W. Blegen in AJA **32**: 141 ff., 1928 is still fundamental.

[53] W. Lamb, *Anatolian Studies pres. to W. H. Buckler*, 132 ff., 149, 1939. Bittel has likewise stressed the essential unity of Anatolia when compared as a whole with the more eastern areas.

AN EXPERIMENT IN MEASUREMENT

ALEXANDRE KOYRÉ

Professor, Ecole Pratique des Hautes Etudes, Sorbonne, Paris

(*Read November 14, 1952*)

HISTORIANS of modern science,[1] when trying to determine its essence and structure, and thus to oppose it to the medieval and classical ones, insist, as often as not, in contradistinction to the abstract and bookish character of the latter, upon the empirical and concrete character of the former. Observation and experience leading a vigorous—and victorious—assault upon tradition and authority: such is the image, itself traditional, that we are usully given of the spiritual revolution of the seventeenth century, of which modern science is, in the same time, the root and the fruit.

This picture is by no means wrong. Quite on the contrary: it is perfectly obvious that modern science has unmeasurably—and even beyond measure—enlarged our knowledge of the world, increased the number of "facts"—all kinds of facts—that it has discovered, observed and collected. Besides, it is just in this way that some of the founders of modern science have seen and understood themselves and their work. Gilbert and Kepler, Harvey and Galileo—they all extoll the admirable fecundity of experience and direct observation, as they oppose it all to the sterility of abstract and speculative thought.[2]

Yet, whatever the importance of the new "facts" discovered and brought together by the *venatores*, a simple amount of "facts," that is, a mere collection of observational or experiential data, does not constitute a science: they have to be ordered, interpreted, explained. In other words, it is only when subjected to theoretical treatment that a knowledge of facts becomes science.

Besides, observation and experience—in the meaning of brute, common-sense observation and experience—had a very small part in the edification of modern science;[3] one could even say that they constituted the chief obstacles that it encountered on its way. It was not *experience*, but *experiment* that had nourished its growth and decided the struggle: the empiricism of the modern science is not *experiential*; it is *experimental*.

I certainly don't need to insist here upon the difference between "experience" and "experiment." Yet I would like to stress the close connection between this latter and the building of theory. Far from being opposed to each other, experiment and theory are bound together and mutually interdetermined, and it is with the growth of precision and refinement of theory that grow the precision and refinement of the experiments. Indeed, an experiment—as Galileo so beautifully has expressed it—being a question put before nature, it is perfectly clear that the activity which results in the asking of this question is a function of the elaboration of the language in which it is formulated. Experimentation is a teleological process of which the goal is determined by theory. The "activism" of modern science, so well noticed—*scientia activa, operativa*—and so deeply misinterpreted by Bacon is only the counterpart of its theoretic development.

We have to add, moreover—and this determines the characteristic features of modern science—that, for its theoretical work, it adopts and develops the pattern of thinking of the mathematician. This is the reason why its "empiricism"

[1] I shall use the term "modern science" for the science of the seventeenth and eighteenth centuries, i.e., for the period which goes, roughly, from Galileo to Einstein. This science is sometimes called "classical" science in contradistinction to the contemporary one; I will not follow this usage and will reserve the designation "classical science" to the science of the classical world, chiefly to that of the Greeks.

[2] *Cf.* for instance W. Whewell, *History of the inductive sciences*, 3 v. London, T. W. Parker, 1837; E. Mach, *Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt*, Leipzig, F. A. Brockhaus, 1883; 9th ed., Leipzig, F. A. Brockhaus, 1933; in English under the title: *The science of mechanics*, Chicago, Open Court, 1883; 5th ed., La Salle, Open Court, 1943.

[3] As recognized already by Tannery and Duhem, the Aristotelian science is in much better accordance with common experience than that of Galileo and Descartes. *Cf.* P. Tannery, Galilée et les principes de la dynamique, in *Mémoires Scientifiques* **6**: 400 sq., Toulouse, E. Privat, 1926; P. Duhem, *Le Système du Monde* **1**: 194–195, Paris, Herrman, 1913.

differs *toto caelo* from that of the Aristotelian tradition:[4] "the book of nature is written in geometrical characters" declared Galileo; this implies that in order to reach its goal modern science is bound to replace the system of flexible and semi-qualitative concepts of the Aristotelian science by a system of rigid and strictly quantitative ones. Which means that modern science constitutes itself in substituting for the qualitative or, more exactly, for the *mixed* world of common-sense (and Aristotelian science) an Archimedian world of geometry made real; or—which is exactly the same thing—in substituting for the world of the more-or-less of our daily life a universe of measurement and precision. Indeed this substitution implies automatically the exclusion from—or the relativation in—this universe of everything that cannot be subjected to exact measurement.[5]

It is this research of quantitative precision, of the discovery of exact numerical data, of these "numbers, weights, measures" upon which God has built the world that forms the goal, and thus determines the very structure of the experiments of modern science. This procedure is not co-extensive to experimentation in general: neither the alchemy, nor Cardano, nor Giambatista Porta —nor even Gilbert—is looking for numerical results. This because they think the world as an ensemble of qualities much more than as an ensemble of magnitudes. Quality, indeed, is repugnant to the precision of measure.[6] Nothing is more significant in this respect than the fact that Boyle and Hooke, both of them experimenters of the first rank, men who know the value of precise measurement, make a purely qualitative study of the spectral colors. Nothing reveals better the incomparable greatness of Newton than his ability to transcend the realm of quality and to break through into the realm of physical, that is quantitatively determined, reality. But besides the theoretical (conceptual) and psychological difficulties that hinder the application of the idea of mathematical rigor to the world of perception and action, the actual performance of a correct measurement encounters in the seventeenth century technical difficulties of which, living as we do in a world overcrowded with, and dominated by, precision instruments, we have, I am afraid, a very bad understanding. Even historians, who—as Professor I. Bernard Cohen pointed out—only too often present us with the decisive experiments of the past not as they *were* actually performed *then*, but as they *are* performed *now* in our laboratories and classrooms, do not realize the real conditions, and therefore the real meaning, of experimentation in the heroic epoch of modern science.[7] And it is in order to bring a contribution to the history of the constitution of the experimental methods of science that I will try, today, to tell the story of the first conscious and sustained attempt of an experimental measurement. The measurement of a universal constant, the constant of acceleration of bodies in their free fall.

Everybody knows the historical importance of the law of fall, the first of the mathematical laws of the new dynamics developed by Galileo, the law which established, once and forever, that "motion is subjected to the law of number."[8] This law presupposes that gravity, though by no means an essential property of bodies (and of which, moreover, we ignore the nature), is, nevertheless, their universal property (all bodies are "heavy" and there are no "light" ones); besides, for every one of them it is an invariable and constant property. It is only on these conditions that the Galilean law is valid (in the vacuum).

Yet, in spite of the mathematical elegance and physical plausibility of the Galilean law, it is obvious that it is not the only possible one.[9] Besides,

[4] It is an empiricism that the Aristotelian tradition opposes to the abstract mathematism of the Galilean dynamics. *Cf.*, on the empiricism of the Aristotelians, J. H. Randall, Jr., Scientific method in the School of Padua, *Jour. Hist. of Ideas* **1**: 177–206, 1940.

[5] This applies, of course, only to the so called "exact sciences" (physico-chemical) in contradistinction to "natural science" or "history" (sciences dealing with the "natural" world of our perception and life) which does not—and perhaps cannot—discard quality and substitute a world of exact measures for the world of the "more-or-less." In any case neither in botany nor in zoology, nor even in physiology and biology did exact measurements play any role; their concepts are still the non-mathematical concepts of the Aristotelian logic.

[6] Quality can be ordered, but not measured. The "more and less" we are using in respect to quality enable us to build a scale, but not to apply exact measurement.

[7] *Cf.* I. Bernard Cohen, A sense of history in science, *Amer. Jour. Physics* **18** (6): 343 sq., 1950.

[8] *Cf.* Galileo Galilei, *Discorsi e Dimostrazioni matematiche intorno a due nuove scienze*, *Opere*, Edizione Nazionale, **8**: 190, Firenze, 1898.

[9] Thus G. B. Baliani proposes a law according to which traversed spaces are *ut numeri* and not *ut numeri impares;* Descartes and Torricelli discuss the possibility of the spaces being in cubical and not in quadratic proportion to the time; in the Newtonian physics acceleration is a function of attraction and therefore not constant. Moreover, as Newton himself does not fail to point out, the inverse square law of attraction is by no means the only possible one.

we are not in the vacuum but in the air, and not in the abstract space, but on the earth, and even, perhaps, on a moving one. It is quite clear that an experimental verification of the law, as well as of its applicability to bodies falling in our space, *in hoc vero aere*, is indispensable. Just as is indispensable the determination of the concrete value of the acceleration (of g).

It is well known with what extreme ingenuity, being unable to perform direct measurements, Galileo substitutes for the free fall the motion on an inclined plane on one hand, and that of the pendulum on the other. It is only justice to recognize his immense merit and genial insight, which are not diminished by the fact that they are based on two wrong assumptions.[10] But it is justice too to turn our attention to the amazing and pitiful poverty of the experimental means at his disposal.

Let us learn from himself his *modus procedendi*:[11]

> A piece of wooden moulding or scantling, about 12 cubits long, half a cubit wide, and three fingerbreadths thick, was taken; on its edge was cut a channel a little more than one finger in breadth; having made this groove very straight, smooth and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth and very round bronze ball. Having placed this board in a sloping position, by lifting one end some one or two cubits above the other, we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent. We repeated this experiment more than once in order to measure the time with an accuracy such that the deviation between two observations never exceeded one tenth of a pulse beat. Having performed this operation and having assured ourselves of its reliability, we now rolled the ball only one quarter the length of the channel; and having measured the time of its descent, we found it precisely one half of the former.
>
> Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or indeed for any fraction; in such experiments repeated a full hundred times we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane, i.e., of the channel along which we rolled the ball. We also observed that the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which, as we shall see later, the author has predicted and demonstrated for them.[12]
>
> For the measurement of time, we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water which we collected in a small glass during the time of each descent, whether for the whole length of the channel or for a part of its length; the water thus collected was weighed, after each descent, on a very accurate balance; the differences and the ratios of these weights gave us the difference and the ratios of the times, and this with such accuracy that although the operation was performed many, many times there was no appreciable discrepancy in the results.

A bronze ball rolling in a "smooth and polished" wooden groove! A vessel of water with a small hole through which it runs out and which one collects in a small glass in order to weigh it afterwards and thus measure the times of descent (the Roman water-clock, that of Ctesebius, had been already a much better instrument): what an accumulation of sources of error and inexactitude!

It is obvious that the Galilean experiments are completely worthless: the very perfection of their results is a rigorous proof of their incorrection.[13]

[10] Galileo's experiments are based on the assumptions (*a*) that the motion of a ball *rolling* down on an inclined plane is equivalent to that of a body *gliding* down (without friction) on the selfsame plane and (*b*) that the pendular motion is perfectly isochronous. This isochronism being a consequence of his law of fall, an experimental confirmation of the former would therefore confirm the latter. Unfortunately, no direct measurement of consecutive oscillation-periods is possible: just because there are no clocks with which we could measure them. Galileo, therefore—and one cannot but admire his experimental genius—substitutes for the direct measurement the comparison of the motion of two different pendula (of equal length), the bobs of which, though having performed oscillations of different amplitudes, arrive nevertheless at the same moment at their position of equilibrium (the lowest point of the curve); the same experiment made with pendula, the bobs of which are constituted by bodies of different weight, demonstrates experimentally that bodies heavy and light (individually as well as specifically) fall with the same speed. *Cf. Discorsi*, 128 sq.

[11] *Cf. Discorsi*, 212 sq. I am quoting the translation of Henry Crew and Alfonso de Savio, *Dialogues concerning two new sciences*, 178 sq., N. Y., Macmillan, 1914; reprinted, N. Y., Dover Publications, 1952.

[12] The speed of the descent is proportional to the sine of the angle of inclination. *Cf. ibid.*, 215, 219; pp. 181, 185 of the translation.

[13] Modern historians, accustomed to see the Galilean experiments performed for the benefit of students in our school laboratories, accept indeed this astonishing report as gospel truth and even praise Galileo for having thus experimentally established not only the empirical validity of the law of fall, but even this law itself. (*Cf.*, among countless others, N. Bourbaki, *Eléments de mathématique* **9**, première partie, livre IV, chap. I–III, Note historique, p. 150 (Actualités scientifiques et industrielles, N 1074, Paris, Herrman, 1949)). *Cf.* Appendix 1.

No wonder that Galileo who, doubtlessly, is fully aware of all that, refrains, as far as possible (thus in the *Discourses*), from giving a concrete value for the acceleration; and that whenever he gives it (as in the *Dialogue*), it is completely and utterly false. So false that Father Mersenne has been unable to hide his surprise: "He supposes," writes he to Peyresc,[14] "that a bullet falls one hundred cubits in 5″ [seconds]; wherefrom it follows that the bullet will fall not more than four cubits in one second, though I am certain that it will fall from a greater height."

Indeed, four cubits—not even seven feet[15]—is less than the half of the true value; and about half the value that Father Mersenne will establish himself. And yet, that the figures given by Galileo are grossly inaccurate is by no means surprising; quite the contrary: it would be surprising, and even miraculous, if they were not. What is surprising, that is the fact that Mersenne, whose experimental means were not much richer than those of Galileo, could have obtained so much better results.

Thus modern science finds itself at its beginnings in a rather strange and even paradoxical situation: it has precision for principle; it asserts that the real is, in its essence, geometrical and, consequently, subject of rigorous determination and measurement (*vice-versa*, mathematicians as Barrow and Newton see in geometry itself a science of measurement[16]); it discovers and formulates (mathematically) laws that allow it to deduce and to calculate the position and speed of a body at each point of its trajectory and at each moment of its motion, and it is not able to use them because it has no ways to determine a moment, nor to measure a speed. Yet, without these measures the laws of the new dynamics remain abstract and void. In order to give them a real content it is indispensable to possess the means of measuring time (space is easy to measure), that is *organa chronou, horologia*, timekeepers as Galileo has called them; in other words: reliable clocks.[17]

Time, of course, cannot be measured directly but only through something else in which we find it embodied. That is either (*a*) a constant and uniform process, such for instance as the constant and uniform motion of the heavenly sphere or the constant and uniform outflow of water in the water-clock of Ctesebius;[18] or (*b*) a process which, though not uniform in itself, can be repeated, or repeats itself, automatically; or finally (*c*) a process which, though not repeating itself as completely identical, employs for its completion the same amount of time, presenting us thus, so to say, an atom or unity of duration.

It is in the pendular motion that Galileo found such a process. Indeed a pendulum, provided of course all external and internal impediments, such, for instance, as friction or the resistance of air, were eliminated, would reproduce and repeat its oscillations, in a perfectly identical manner, till the end of time. Moreover, even *in hoc vero aere* where its motion is continuously retarded and where no two oscillations are strictly identical, the period of these oscillations remains constant.

Or to put it in Galileo's own words:[19]

> First of all one must observe that each pendulum has its own time of vibration so definite and determinate that it is not possible to make it move with any other period than that which nature has given it, and which depends neither on the weight of bob, nor on the amplitude of the oscillation, but only and solely on the length of the pendulum.

[14] F. M. Marin Mersenne, *Lettre à Peyresc* of 15 January 1635; *cf.* Tamizey de Larroque, *La Correspondance de Peyresc* **19**: 112, Paris, A. Picard, 1892; *cf. Harmonie Universelle* **1** (*2*): 85, 95, 108, 112, 144, 156, 221, Paris, 1636.

[15] The florentine cubit, doubtlessly used by Galileo, contains 20 inches, i.e., 1 foot, 8 inches; and the Florentine foot is equal to the Roman one, that is equal to 29.57 cm.

[16] *Cf.* Isaak Barrow, *Lectiones Mathematicae* of 1664–66 (*The Mathematical Works of Isaac Barrow, D. B.*, ed. by W. Whewell, Cambridge, C. U. P. 1860), 216 sq; Isaac Newton, *Philosophiae naturalis principia mathematica, preface*, London, 1687.

[17] The unreliability of the clocks of the sixteenth and seventeenth centuries is well known; precision clocks are byproducts of scientific development (*cf.* Willis I. Milham, *Time and timekeepers*, N. Y., Macmillan, 1923; L. Defossez, *Les savants du XVIIe siècle et la mesure du temps*, Lausanne, ed. *Journal Suisse d'horlogerie*, 1946), yet their building is usually explained by the urge of solving the problem of longitude, i.e., the pressure of practical needs of navigation, the economical importance of which grew considerably since the circumnavigation of Africa and the discovery of America (*cf.* for instance Lancelot Hogben, *Science for the citizen*, 2nd ed., 7th impression, 235 sq.; London, G. Allen and Unwin, 1946). Without denying the importance of practical needs or economic factors on the development of science, I believe this explanation, which combines Baconian and Marxist prejudices for *praxis* against *theoria*, to be at least 50 per cent false: the motives for building correct time measuring instruments were, and still are, immanent to the scientific development itself. *Cf.* my paper, Du monde de l'à peu près à l'univers de la précision, *Critique*, n. 28, 1946.

[18] *Cf.* its description in H. Diels, *Antike Technik*, 3rd ed., Leipzig, Teubner, 1924.

[19] *Cf.* Galileo Galilei, *Discorsi e dimostrazioni*, 141; English translation, 141.

This great discovery, by the way, has been made by Galileo not by gazing at the oscillations of the great candelabra of the cathedral of Pisa and stating their isochronism by comparing them with the beats of his pulse, as following Viviani is still told in the textbooks,[20] but by extremely ingenious experiments in which he compares the oscillations of two pendula of the same length but with bobs of different matter and thus of different weight (cork and lead),[21] and first and foremost by hard mathematical thinking. Thus says Salviati:[22]

> And first, as to the question whether one and the same pendulum really performs its vibrations, large, medium and small, all in exactly the same time, I shall rely upon what I have already heard from our Academician. He has clearly shown that the time of descent is the same along all chords, whatever the arcs which subtend them, as well along an arc of 180° [i.e., the whole diameter] as along one of 100°, 60°, 10°, 2°, ½° or 4′. It is understood, of course, that these arcs all terminate at the lowest point of the circle, where it touches the horizontal plane.
>
> If now we consider descent along arcs instead of chords then, provided these do not exceed 90°, experiment shows that they are all traversed in equal times; but these times are greater for the chord than for the arc, an effect which is all the more remarkable because at first glance one would think just the opposite to be true. For since the terminal points of the two motions are the same and since the straight line included between these two points is the shortest distance between them, it would seem reasonable that the motion along this line should be executed in the shortest time; but this is not the case, for the shortest time—and therefore the most rapid motion—is that employed along the arc of which this straight line is the chord.
>
> As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the threads; or one might say the lengths are to each other as the squares of the times; so that if one wishes to make the vibration-time of one pendulum twice that of another, he must make its suspension four times as long. In this manner, if one pendulum has a suspension nine times as long as another, this second pendulum will execute three vibrations during each one of the first; from which it follows that the lengths of the suspending chords bear to each other the (inverse) ratio of the squares of the number of vibrations performed in the same time.

One cannot but admire the depth of the Galilean thinking which shows itself in its very error: the oscillations of the pendulum are, of course, not isochronous; and the circle is not the line of the quickest descent; but, to use the terms of the eighteenth century, the "brachistochrone" curve, and the curve upon which oscillations are performed in the same time, or the "tautochrone" one, are recognized by Galileo to be the same line.[23]

It is rather strange that, having discovered the isochronism of the pendulum—the very basis of all modern chronometry—Galileo, though he tried to achieve with its help a time keeper, and even to construct a mechanical pendulum-clock[24]—never used it in his own experiments. It seems that it was Father Mersenne who first got this idea.

Mersenne, as a matter of fact, does not tell us *expressis verbis* that he employed the pendulum as a means of measuring the time of descent of heavy bodies in the experiments he reports about in his *Harmonie Universelle*.[25] But as in the same work he gives a careful description of the motion of the semi-circular pendulum and insists upon the vari-

[20] The famous candelabra was put into the Cathedral of Pisa three years after Galileo's departure from this city; at the time, in which Viviani places the discovery, the cupola of the Pisan Cathedral was still bare and void. *Cf.* E. Wohlwill, Ueber einen Grundfehler aller neueren Galilei-Biographien, *Münchener medizinische Wochenschrift*, 1903, and *Galilei und sein Kampf für die Copernicanische Lehre* **1**, Hamburg und Leipzig, L. Voss, 1909; R. Giacomelli, *Galileo Galilei Giovane e il suo "De Motu."* Quaderni di storia e critica della scienza, **1**, Pisa, 1949.

[21] *Cf. supra*, n. 10.

[22] *Cf.* Galileo Galilei, *Discorsi e dimostrazioni*, 139; English translation, 95.

[23] The times of descent on all the chords being equal and the motion along the (circular) arc being quicker than that along the chord, it was reasonable for Galilei to assume that the descent along the arc was the quickest possible and that the motion of the pendulum was, therefore, isochronous. That it is not the case was discovered experimentally by Mersenne in 1644 (*cf. Cogitata Physico-Mathematica, Phenomena Ballistica*, Parisiis, 1644, propositio XV, septimo, p. 42), and theoretically by Huygens who, in 1659, demonstrated that the "tautochrone" line of descent is the cycloid and not the circle (the same discovery was made independently by Lord Brounker, in 1662). As for the cycloid being at the same time the curve of the quickest descent ("brachistochrone"), this was demonstrated by J. Bernoulli in 1696, and independently—answering the challenge of Bernoulli—by Leibniz, de l'Hopital and Newton.

[24] This clock, or, more exactly, its central regulating mechanism, was constructed by Viviani; *cf. Lettera di Vincenzio Viviani al Principe Leopoldo de' Medici intorno al applicazione del pendolo all' orologio*, in Galileo Galilei, *Opere*, Ed. Naz., **19**: 647 sq, Firenze, 1907; *cf.* equally E. Gerland-F. Traumüller, *Geschichte der physikalischen Experimentierkunst*, 120 sq., Leipzig, W. Engelmann, 1899; L. Defossez, *op. cit.*, 113 sq.

[25] *Cf. Harmonie Universelle* **1**: 132 sq., Paris, 1636.

ous utilizations of the same in medicine (for the determination of the variations in speed of the pulse beats), in astronomy (for the observation of the eclipses of the moon and the sun), etc.,[26] it is practically certain, and moreover confirmed by another passage of the *Harmonie Universelle*, not only that he did use a pendulum but even that this pendulum was three and a half feet long.[27] It is indeed of such a pendulum that, according to Mersenne, the period is exactly equal to one second of the prime mobile.[28]

The results of Mersenne's experiences, "performed more than 50 times," are quite consistent: the falling body traverses 3 feet in half a second, 12 in a second, 48 in two, 108 in three, and 147 in three and a half. Which is nearly twice as much (80 per cent) as it should be according to the figures given by Galileo.

Thus Marsenne writes:[29]

> But concerning the experiences of Galileo, one cannot imagine where the great difference that one finds here in Paris and in the surroundings concerning the times of the falls, which have always appeared to us to be much smaller than his, comes from: not that I should like to reproach such a great man for little care in his experiences, but we have made ours many times from different heights, in the presence of many persons and they always succeeded in the same way. Therefore if the cubit which Galileo has used has only one foot and two thirds, that is twenty inches of the royal foot which we use in Paris, it is certain that the bullet descends more than one hundred cubits in 5 seconds.

Indeed, explains Mersenne, "the hundred cubits of Galileo" are equal to 166⅔ of "our" feet.[30] But Mersenne's own experiments "repeated more than fifty times" have given quite different results; according to them, in 5″ a heavy body will traverse not 100, but 180 cubits or 300 feet.

Mersenne does not tell us that he has actually dropped heavy bodies from the altitude of 300 feet: it is a conclusion that he draws by applying the "duplicate proportion" to the experimental data at his disposal. Yet as these data "demonstrate" that a heavy body falls three feet in half a second, twelve in a second, forty-eight in two, 108 in three and 147 in three and a half[31]—figures that are in a perfect accord with the duplicate proportion—Mersenne feels entitled, and even bound, to assert that a heavy body will fall 166⅔ feet in only $3^{18}/_{25}$ seconds, and not in five. Moreover, he adds, from Galileo's figures, it would follow that a

[26] *Ibid.*, 136: "Quoy qu'il en soit, cette manière d'Horologe peut servir aux observations des Eclypses de Soleil, & de la Lune, car l'on peut conter les secondes minutes par les tours de la chorde, tandis que l'autre fera les observations, & marquer combien il y aura de secondes, de la première à la troisiesme observation, etc.

"Les Médecins pourront semblablement user de cette methode pour reconnoitre de combien le poux de leur malades sera plus viste ou plus tardif à diverses heures, et divers iours, et combien les passions de cholere, et les autres le hastent ou le retardent; par exemple qu'il faut une chorde de trois peids de long pour marquer la durée du poux d'aujourd'hui par l'un de ses tours, et qu'il en faille deux, c'est à dire un tour et un retour pour le marquer demain, ou qu'il ne faille plus qu'une chorde longue de 3/4 de pied pour faire un tour en mesme temps que le poux bat une fois, il est certain que le poux bat fois plus viste."

[27] *Ibid.*, 220, Corollaire 9: "Lorsque j'ay dit que la chorde de 3 pieds & demy marque les secondes par les tours ou retours, je n'empesche nullement que l'on n'accourcisse la chorde, si l'on trouve qu'elle soit trop longue, et chacun de ses tours dure un peu trop pour une seconde, comme i'ay quelquefois remarqué, suivant les différentes horologes communes ou faites exprez: par exemple le mesme horloge commun, dont i'ay souvent mesuré l'heure entière avec 3600 tours de la chorde de 3 pieds & demy, n'a pas fait d'autresfois son heure si longue: car il a fallu seulement faire la chorde de 3 pieds pour avoir 900 retours dans l'un des quarts d'heure dudit horologe: et i'ay experimenté sur une monstre à roue faite exprez pour marquer les seules secondes minutes, que la chorde de 2 pieds & demi ou environ faisoit les tours esgaux ausdites secondes. Ce qui n'empesche nullement la vérité ny la iustesse de nos observations, à raison qu'il suffit de sçavoir que les secondes dont ie parle, sont esgales à la durée des tours de ma chorde de 3 pieds & demy: de sorte que si quelqu'un peut diviser le jour en 24 parties esgales, il verra aisément si ma seconde dure trop, et de combien est trop longue." For his subsequent experiments reported in *Cogitata Physico-Mathematica, Phenomena Ballistica*, 38 sq., Mersenne used a pendulum of three feet only. He had noticed, indeed, that the three and a half feet one was a bit too long, though the difference was practically imperceptible; *cf. Cogitata*, 44.

[28] "One second of the prime mobile" is the time in which the "prime mobile," i.e., the skies, or the earth, describes a rotation of one second.

[29] *Cf. Harmonie Universelle* **1**: 138.

[30] As a matter of fact, the foot used by Galileo is shorter—29.57 cm.—than the "royal" foot used by Mersenne—32.87 cm.; the difference of their respective data is therefore even much greater than it is assumed by the latter.

[31] As a matter of fact, Mersenne obtained 110 and not 108 feet on one hand, and 146½ on the other. But Mersenne does not believe in the possibility of reaching exactitude by experiment—considering the means at his disposal, he is perfectly right—and thus assumes that he is entitled to correct the experimental data in order to fit them to the theory. Once more he is perfectly right, as long, of course, as he remains (and he does) on this side of the margin of the experimental errors. Needless to say that Mersenne's procedure has been followed by science ever since. *Cf.* Appendix 2.

heavy body would fall only one cubit in half a second, and four cubits, that is about 6⅔ feet in one second, instead of the twelve feet which it descends in fact.

The results of the Mersennian experiments—the figures obtained by him—of which he is very proud, and of which he makes use for calculating the times with which bodies would fall from all possible altitudes up to the moon and the stars,[32] and the length of all kinds of pendulums with periods up to 30″—constitute, undoubtedly, a progress in respect to those of Galileo. Yet they imply a rather awkward consequence, opposed not only to common sense and the fundamental teachings of mechanics, but also to Mersenne's own calculations: namely that the descent on the periphery of the circle is quicker than that on the perpendicular.[33]

Mersenne seems not to have noticed this consequence (nor did anybody else) at least for several years. In any case he does not mention it before the *Cogitata Physico-Mathematica* of 1644, where, resuming anew the discussion of the law of the fall and of the properties of the pendulum, he states it, though in somewhat attenuated form, together with that of the non-isochronism of the great and small oscillations.[34]

Thus, having explained how strange it is that a three-foot pendulum (which he uses now instead of the three-and-a-half-foot one which he employed formerly) makes his semi-oscillation in exactly half a second (that is, descends three feet), when free-falling bodies traverse twelve feet in a second (that is equally three feet in a semi-second), whereas according to calculations made already in the *Harmonie Universelle* it should traverse in the time of a semi-oscillation 11/7 of the semi-diameter[35] (i.e., 33/7 or 5 feet) he continues,

> this implies a very great difficulty, because both [these facts] have been confirmed by numerous observations, namely that falling bodies traverse on the perpendicular twelve feet only, and that the three feet pendulum descends from *C* to *B* in half a second; which cannot occur but if the globe [of the pendulum] descends from *C* to *B* on the circumference in the same time as a similar globe [falls] on the perpendicular *AB*. Now as this one should descend 5 feet in the time in which the globe comes from *C* to *D*, I do not see any solution.

One could of course assume that bodies fall quicker than it has been admitted: but this would be against all observations. We have therefore, states Mersenne, either to accept that bodies fall on the perpendicular with the same speed as they descend on the circle, or that the air resists more strongly the motion downwards than the oblique one, or finally, that bodies traverse in free fall more than 12 feet in one second, and more than 48 in two, but that, because of the difficulty of ascertaining exactly, by attending to the sound of the percussion of the body on the pavement, the precise moment of this occurrence, all our observations concerning this question are utterly faulty.[36]

It must have been rather hard for Mersenne to admit the fallaciousness of his, so carefully made, experiments and the meaninglessness of the long calculations and tables based upon them. Yet it was unavoidable. Once more he had to recognize that precision couldn't be achieved in science and that its results were only approximately valid. Thus it is not surprising that in his *Reflexiones Physico-Mathematicae* of 1647 he tries in the same time to perfect his experimental methods—thus by holding the bob of the pendulum and the descending body (similar leaden spheres) in one and the same hand in order to insure the simultaneity of the beginning of their motions,[37] and by fixing his pendulum to a wall in order to insure the simultaneity of the end of these motions by the merging together of the two sounds produced by the hit of the pendulum upon the wall and by that of the falling body upon the ground; and to explain, at a considerable length, the lack of certainty of the results,[38] which, by the way, confirm those of his former investigations: the body seems to fall 48 feet in about 2″, and 12 in 1″. Yet, insists Mersenne, it is impossible to determine exactly the length of the pendulum of which the period would be precisely a second, nor is it possible to perceive, by hearing, the exact coincidence of the two sounds. A couple of inches or even feet more or

[32] *Cf. ibid.*, 140. In his calculations, Mersenne assumes—as Galileo—that the value of the acceleration is a universal constant.

[33] The ball descends on the quadrant of the circle as quickly as on the radius, if this radius is equal to 3 feet, or even quicker if the radius is equal to 3½ feet.

[34] *Cf. Cogitata Physico-Mathematica, Phenomena Ballistica*, 38 and 39; see Appendix 3.

[35] *Cf. ibid.*, 41.

[36] It is interesting to note that in his experiments, Mersenne determines the moment of arrival of the falling body to the earth not by sight, but by hearing; the same method will be followed by Huygens, doubtlessly under Mersenne's influence.

[37] *Cf. Reflexiones Physico-Mathematicae* **18**: 152 sq., Parisiis, 1647.

[38] *Cf. ibid.* **19**: 155: *De variis difficultatibus ad funependulum et casum gravium pertinentibus.*

less does not make any difference. Thus, he concludes, we have to content ourselves with approximations and not ask for more.

Nearly at the same time in which F. M. Mersenne performed his experiments, another experimental research of the laws of fall, linked together with an experimental determination of the value of g, was made in Italy by a team of Jesuit scientists led by the famous author of the *Almagestum Novum*, R. P. Giambattista Riccioli,[39] who, strangely enough, was perfectly independent from, and even wholly ignorant of, the work of Mersenne.

Riccioli has a rather bad reputation with the historians of science—a reputation not quite merited.[40] Yet one must confess that he is not only a much better experimenter than F. M. Mersenne, but even a much more intelligent one, and that he has an infinitely deeper understanding of the value and meaning of precision than the friend of Descartes and Pascal.

It was in 1640, when he was professor of philosophy in the *Studium* of Bologna, that Riccioli started a series of investigations of which I shall give here a brief account,[41] and I would like to stress the carefully thought out and methodical way in which he proceeds with his work. He does not want to take anything for granted and, though, as a matter of fact, he is firmly convinced of the value of Galileo's deductions, he first tries to establish, or better to say, to verify, whether the thesis of the isochronism of pendular oscillations is exact; then, whether the relation asserted by Galileo between the length of the pendulum and its period (period proportional to the square root of the length) is confirmed by experience; finally, to determine, as precisely as possible, the period of a given pendulum in order to obtain in this way a time-measuring instrument fit to be used for the experimental research of the speed of fall.

Riccioli starts by preparing a convenient pendulum: a spherical metallic bob suspended to a chain[42] attached to a metallic cylinder turning freely in two, equally metallic, sockets. A first series of experiments aims at the verification of the Gallilean assertion of the constancy of the period of the pendulum by counting the number of its oscillations in a given time. The time is measured by the means of a water-glass and Riccioli, revealing a deep understanding of the empirical conditions of experimentations and measurement, explains that it is the double process of running out and filling again of the water-glass that is to be taken as the unit of time. The results of this first series confirm the assertion of Galileo.

A second series of experiments for which Riccioli uses two pendulums, of the same weight, but of different length ("height"), namely of one and of two feet, confirms the square root relation established by Galileo: the number of oscillations in the unit of time is, respectively, 85 and 60.[43]

Mersenne would probably stop at this point. Not Riccioli; he understands quite well that even by using his method of turning the water-glass upside down one is still far away from real precision: for this we have still to look elsewhere, that is to the skies, to the only really exact *horologium* existing in this world, to the *organa chronou* provided by nature, the motions of the heavenly bodies and spheres.

Riccioli realizes full well the tremendous importance of the Galilean discovery: the isochronism of the pendulum enables us to achieve a *precise* time keeper. Indeed, the fact that large and small oscillations are performed in the same time entails the possibility of maintaining its motion as long as we want by counteracting its normal and spontaneous slowing down; for instance, by giving it a new push after a certain amount of beats;[44] thus

[39] The report on these experiments is included in the *Almagestum Novum, Astronomiam veterem novamque complectens observationibus aliorum et propriis, Novisque Theorematibus, Problematibus ac Tabulis promotam . . . auctore P. Johanne Baptista Riccioli Societatis Jesu . . .*, Bononiae, 1651. The work had to have three volumes, but only the first one, in two parts, has been published. This "first volume" is, indeed, 1504 pages long (in folio).

[40] Riccioli is, of course, an anti-Copernican and, in his great works—*Almagestum Novum* of 1651 and *Astronomia Reformata* of 1665—he heaps arguments upon arguments in order to refute Copernicus, which is, indeed, regrettable, but after all rather natural for a Jesuit. On the other hand, he does not hide his great admiration for Copernicus and Kepler and gives a surprisingly correct and honest account of the astronomical theories he is criticizing. He is immensely learned and his works, especially the *Almagestum Novum*, are an invaluable source of information. This makes his ignorance of the works of Mersenne so much more surprising.

[41] *Cf. Almagestum Novum* **1** (1), bk. II, ch. XX and XXI: 84 sq. and **1** (2), bk. IX, sect. IV, 2: 384 sq. I presented a report on the experiments of Riccioli to the *XXII Congrès international de Philosophie des Sciences* which met in Paris, in 1949.

[42] *Cf. Almagestum Novum* **1** (1), bk. II, ch. XX: 84,

[43] *Cf. ibid.*, ch. XXI, prop. VIII: 86.

[44] This pushing of the pendulum is by no means easy and implies a long training.

any number of atoms of time can be accumulated and added together.

It is clear, however, that in order to be able to use the pendulum as a *precise* instrument for measuring time, we have to determine *exactly* the value of its period. This is the task to which, with an unyielding patience, Riccioli will devote himself. His aim is to manufacture a pendulum of which the period would be exactly one second.[45] Alas, in spite of all efforts he will not be able to reach his goal.

To begin with he takes a pendulum weighing about one pound and three feet and four inches (Roman) [46] "high." The comparison with the water-glass has been satisfactory: nine hundred oscillations in a quarter of an hour. Riccioli proceeds, then, to a verification by the means of a sundial. For six consecutive hours, from nine o'clock in the morning to three o'clock in the afternoon, he counts (he is aided by the R. P. Francesco Maria Grimaldi) the oscillations. The result is disastrous: 21,706 oscillations instead of 21,660. Moreover, Riccioli recognizes that for his aim the sundial itself lacks the wanted precision. Another pendulum is prepared and "with the aid of nine Jesuit fathers," [47] he starts counting anew; this time—the second of April 1642—for twenty-four consecutive hours, from noon to noon: the result is 87,998 oscillations whereas the solar day contains only 86,640 seconds.

Riccioli makes then a third pendulum, lengthening the suspension chain to 3 feet, 4.2 inches. And, in order to increase the precision even more, he decides to take as a unit of time not the solar, but the sidereal day. The count goes on from the passage through the meridian line of the tail of the Lion (the twelfth of May 1642) till its next passage on the thirteenth. Once more a failure: 86,999 oscillations instead of 86,400 that there should have been.

Disappointed yet still unbeaten, Riccioli decides to make a fourth trial, with a fourth pendulum, somewhat shorter this time, of 3 feet, and 2.67 inches only.[48] But he cannot impose upon his nine companions the dreary and wearisome task of counting the swings. Father Zeno and Father F. M. Grimaldi alone remain faithful to him to the end. Three times, three nights, the nineteenth and the twenty-eighth of May and the second of June 1645, they count the vibrations from the pasage through the meridian line of the Spica (of Virgo) to that of Arcturus. The numbers are twice 3,212 and the third time 3,214 for 3,192 seconds.[49]

At this point Riccioli seems to have had enough of it. After all, his pendulum, the period of which is equal to 59.36‴, is a perfectly usable instrument. The transformation of the number of oscillations into seconds is easy. Besides, it can be facilitated by precalculated tables.[50]

Still, Riccioli is rather worried about his lack of success. He tries, therefore, to calculate the "height" of a pendulum which would swing in exactly a second: arriving at the result that it should be 3 feet, 3.27 inches.[51] He confesses, however, not having actually made it. On the other hand he has certainly manufactured much shorter pendulums in order to achieve a greater refinement in measuring time intervals: one of 9.76 inches with the period of 30‴; another, still shorter, of 1.15 inches of which the period is only 10‴.

"It is such a pendulum that I employed," says Riccioli, "for measuring the speed of the natural descent of heavy bodies" in the experiments performed in this same year 1645 at the Torre degli Asinelli, in Bologna.[52]

Now it is obviously impossible to use so rapid a pendulum simply by counting its swings; one has to find out some means of summing them up. In other words, one has to construct a clock. Actually it is a clock, the first pendulum clock, that Riccioli has built for his experiments. Yet it would be difficult to consider him a great clockmaker, a forerunner of Huygens and Hooke. His clock, indeed, had neither weight nor spring, nor even hands or dial. As a matter of fact, it was not a mechanical clock, but a human one that he built.

In order to sum up the beats of his pendulum

[45] Riccioli, as we shall see, is not as easily satisfied as Mersenne.

[46] A Roman foot is equal to 29.57 cm.

[47] *Cf. Almagestum Novum, loc. cit.*, 86. The names of these fathers ought to be preserved as examples of devotion to science; here they are (*cf.* **1** (*2*): 386): Stephanus Ghisonus, Camillus Rodengus, Jacobus Maria Palavacinus, Franciscus Maria Grimaldus, Vicentius Maria Grimaldus, Franciscus Zenus, Paulus Casarus, Franciscus Adurnus, Octavius Rubens.

[48] *Cf. ibid.*, 87.

[49] *Cf. ibid.*, 85. As the motion of the pendulum is not isochronous the exquisite concordance of the results of Riccioli's experiments can be explained only if we assume that he made his pendulums perform practically equal and *small* oscillations.

[50] Riccioli gives these tables in the *Almagestum Novum* **1** (1): bk. 2, ch. XX, prop. XI: 387.

[51] *Cf. ibid.* and **1** (*2*): 384.

[52] *Cf. ibid.* **1** (1): 87.

Riccioli imagined a very simple, and a very elegant device: he trained two of his collaborators and friends, "gifted not only for physics but also for music, to count *un, de, tre* . . . (in the Bolognese dialect in which these words are shorter than in Italian) in a perfectly regular and uniform way, as are wont to do those who direct the execution of musical pieces, in such a way that to the pronunciation of each figure corresponded an oscillation of the pendulum."[53] It is with this "clock" that he performed his observations and experiments.

The first question studied by Riccioli concerned the behavior of "light" and "heavy" bodies.[54] Do they fall with the same, or with different, speeds? A very important, and very controversial, question, to which, as we know, ancient and modern physics gave different answers. Whereas the Aristotelians maintained that bodies fall so much quicker as they are heavier, Benedetti had taught that all bodies, at least all bodies possessing an identical nature, i.e., specific gravity, fell with the same speed. As for the moderns, such as Galileo and Baliani, followed by the Jesuits Vendelinus and N. Cabeo, they asserted that all bodies, whichever their nature or weight, fell always with the same identical speed (in the *vacuum*).[55]

Riccioli wants to settle this problem once and forever. Thus on the fourth of August 1645 he proceeds to work. Spheres of equal size but of different weight, made, respectively, of clay and of paper, covered with chalk (this is in order to make their motion along the wall, as well as their bursting when reaching the pavement, easier to observe), were dropped from the summit of the Torre dei Asinelli, particularly convenient for this kind of experiment[56] and sufficiently high—312 Roman feet—to make such differences in speed perceptible in their effects. The results of the experiments, which Riccioli repeats fifteen times, are indubitable: heavy bodies fall quicker than light ones. Yet their lagging behind, which, depending on the weight and the dimension of the balls varies from 12 to 40 feet, does not contradict the theory developed by Galileo: it is to be explained by the resistance of the air and has been foreseen by him. On the other hand the observed facts are perfectly incompatible with the teachings of Aristotle.[57]

Riccioli is intensely conscious of the originality and of the value of his work. Accordingly he pokes fun at the "semi-empiricists" who don't know how to make a really conclusive experiment and who, for instance, assert—or deny—that bodies fall with the same speed because they are unable to determine the precise moment when the body strikes the pavement.[58]

The second problem investigated by Riccioli is even more important. He wants to ascertain the proportion with which the falling body accelerates its motion. It is, as it is taught by Galileo, a "uniformly difform" (uniformly accelerated) motion, that is a motion in which the spaces traversed are *ut numeri impares ab unitate* or, as Baliani wants it, a motion in which these spaces are a series of natural numbers? As for the speed, is it proportional to the duration of the fall, or to the space traversed.[59]

Aided by R. P. Grimaldi, Riccioli manufactures a number of balls made of chalk, of identical dimensions and weight, and, after having established by direct measurement of the times of their falling from different stories of the Torre dei Asinelli that they follow the Galilean law,[60] he proceeds to the verification of this result (nothing is more characteristic than this inversion of the procedure) by dropping these balls from previously calculated, determined, altitudes, using to this purpose all the churches and towers of Bologna of which the heights are appropriate, namely, those of St. Peter, St. Petronio, St. James, and St. Francis.[61]

The results are concordant in all details. Indeed their accord is so perfect, the spaces traversed by the balls (15, 60, 135, 240 feet) confirm the Galilean law in so rigorous a manner that it is quite obvious that the experimenters have been convinced of its truth before starting. Which, after all, is not surprising, as the experiments with

[53] *Cf. ibid.* **1** (*2*) : 384.

[54] Riccioli, a hundred years behind his time, still believes in "lightness" as an independent quality correlated with, and opposed to, "heaviness."

[55] *Cf. ibid.*, 387.

[56] The Torre degli Asinelli possess vertical walls and stands on a rather large and flat platform.

[57] *Cf. ibid.*, 388.

[58] *Cf. ibid.* and **1** (1) : 87.

[59] *Cf. ibid.* It is interesting to note that Riccioli uses the old scholastic terminology and, quite correctly, identifies the "uniformly difform" (*uniformiter difformis*) motion with the uniformly accelerated (or retarded) one.

[60] He tells us, indeed, that he thought about the problem since 1629 and adopted the relation 1, 3, 9, 27, before having read Galileo in 1634, having been allowed to do so by his superiors. It is interesting to note that before having read Galileo the very learned Riccioli did not identify the *uniformiter difformis* motion with that of the fall.

[61] *Cf. ibid.*, 387. The experiments were continued from 1640 to 1650.

the pendulum have already given to it a full confirmation.

Yet even if we admit—as we must—that the good fathers corrected somewhat the actual results of their measurements, we have nevertheless to acknowledge that these results are of a quite surprising precision. Compared to the rough approximations of Galileo himself, and even to those of Mersenne, they constitute a decisive progress. They are certainly the best ones that could be obtained by direct observation and measurement and one cannot but admire the patience, the conscience, the energy, and the passion for truth of the R. P. Zeno, Grimaldi, and Riccioli (as well as of their collaborators) who, without any other instrument for measuring time than the human clock into which they transformed themselves, were able to determine the value of the acceleration, or, more exactly, the length of the space traversed by a heavy body in the first second of its free fall through the air, as being equal to 15 (Roman) feet. A value which Huygens alone, by using the mechanical clock invented by him, or better to say, by applying indirect methods which his mathematical genius enabled him to discover and to use in the very construction of his clock, will be in a position to improve.

It is very interesting, and very instructive, to study the *modi procedendi* of the great Dutch scientist to whom we owe our watches and clocks. Their analysis enables us to witness the transformation of the still empirical or semi-empirical experiences of Mersenne and Riccioli into a truly scientific experiment; it imparts to us, too, a very important lesson, namely, that in scientific investigations the direct approach is by no means the best nor the easiest one, and that empirical facts are to be reached only by using a theoretical circuit.

Huygens starts his work by repeating, in 1659 (the twenty-first of October), the (last) experiments of Mersenne, as described by the latter in his *Reflexiones* of 1647; and once more we are obliged to stress the appalling poverty of the experimental means at his disposal: a string-pendulum attached to the wall; its bob, a leaden ball, and another, similar, leaden, globe are held in the same hand; the simultaneity of the arrival of the two globes, respectively, to the wall and to the ground is determined by the coincidence of the two sounds produced by the hits. Strangely enough, using exactly the same procedure as Mersenne, Huygens obtains better results; according to him, the body falls 14 feet.[62]

On the 23rd of October 1659, Huygens repeats the experiment, using this time a pendulum the semi-vibration of which is equal not to a half second, but to three-quarters of it. During this time the leaden sphere falls 7 feet, 8 inches. It follows that in one second it would fall about 13 feet, 7½ inches.[63]

On the fifteenth of November, 1659, Huygens makes a third trial. This time he improves somewhat his procedure by attaching both bob and the leaden sphere to a thread (instead of holding them in the same hand) by the cutting of which they are released. Moreover, he puts parchment on the wall and the ground to make the perception of the sounds more distinct. The result is about 8 feet, 9½ inches. Yet, just as Mersenne before him, Huygens is obliged to admit that his result is valid only as an approximation, because three or even four inches more or less in the height of the fall cannot be distinguished by the means employed by him: the sounds seem to coincide. It follows, therefore, that an exact measure cannot be obtained in this way. But the conclusion he draws therefrom is by no means the same. Quite on the contrary. Whereas Mersenne renounces the very idea of scientific precision, Huygens reduces the role of the experiment to that of verification of theoretically arrived at results. It is enough when it does not contradict them, as in this case where the observed figures are perfectly compatible with those deduced from the analysis

[62] *Cf.* Ch. Huygens, *Œuvres* **17**: 278, La Haye, M. Nijhof, 1932: "II D. 1. Expertus 21 Oct. 1659. Semisecundo minuto cadit plumbum ex altitudine 3 pedum et dimidij vel 7 pollicum circiter. Ergo unius secundi spatio ex 14 pedem altitudine."

[63] *Cf.* Huygens, *Œuvres* **17**: 278. "II D. 2. Expertus denuo 23 Oct. 1659. Pendulum adhibui cujus singulae vibrationes 3/2 secundi unius. unde semivibratio qua usus sum erat 3/4." Erat penduli longitudo circiter 6 p. 11 unc. Sed vibrationes non ex hac longitudine sed conferendo eas cum pendulo horologij colligebam. Illius itaque semivibratione cadebat aliud plumbum simul e digitis demissum ex altitudine 7 pedum 8 unc. Ergo colligitur hinc uno secundo casurum ex altitudine 13 ped. 7 1/2 unc. ferè.

"Ergo in priori experimento debuissent fuisse non toti 3 ped. 5 poll.

"Sumam autem uno secundo descendere plumbum pedibus 13. unc. 8. Mersenne 12 ped. paris. uno secundo confici scribit. 12 ped. 8 unc. Rhijnland. Ergo Mersenni spatium justo brevius est uno pede Rhijnl.

A Rheinland foot is equal to 31.39 cm.

of the motion of the circular pendulum, i.e., about 15 feet, 7½ inches per second.[64]

As a matter of fact, the analysis of the pendular motion gives, as we shall see, even better results.

I have already mentioned the paradoxical situation of modern science at the time of its birth: possession of exact mathematical laws combined with the impossibility of their application because of the inability of performing a precise measurement of the fundamental magnitude of dynamics, that is, of time.

Nobody seems to have felt it more strongly than Huygens, and it is certainly for that reason, and not for practical considerations such as the necessity of good clocks for navigation—though he, by no means, neglected the practical aspect of the question[65]—that, at the very beginning of his scientific career, he applied himself to the solution of this fundamental and preliminary problem: the perfecting, or better, the building of a perfect time-keeper.

It is in the year 1659, the same year in which he made the measurements I have just reported, that he reached his goal by constructing an improved pendulum clock;[66] a clock which he used in order to determine the exact value of the oscillation of the pendulum that he had employed in his experiments.

In the history of the scientific instruments Huygens' clock occupies a very important position: it is the first apparatus that embodies in its construction the laws of the new dynamics; it is the result not of empirical trial and error, but of careful and subtle theoretical investigation of the mathematical structure of circular and oscillatory motions. Thus the very history of the pendulum-clock gives us a good example of the value of the roundabout way in preference to the direct one.

Huygens, indeed, is perfectly aware that, as already discovered by Mersenne, small and large oscillations of the pendulum are not performed in the same time. In order to construct a perfect time-keeper one has, therefore, (*a*) to determine the truly isochronous curve, and (*b*) to find out the means to make the bob of the pendulum to move along this line and not along the periphery of the circle. It is well known that Huygens succeeded in solving both problems (though for doing it he had to devise a completely new geometrical theory),[67] and to achieve a perfectly isochronous motion, the motion along the cycloid; moreover, that he succeeded in fitting his cycloidal pendulum into a clock.[68]

He was now in position to proceed, with an infinitely better equipment—a mechanical clock instead of a human one—and therefore a much better chance of reaching precision, to experiments on the line of those of Riccioli. Yet he never tried to perform them. This because the construction of the pendulum-clock put into his hands a much better procedure.

As a matter of fact, he had not only discovered the isochronism of the motion along the cycloid, but also—something that Mersenne had tried (but failed) to find out for the circle—the relation between the time of descent of a body along the cycloid to that of its fall along the diameter of its

[64] *Cf. ibid.*; 281: "II D. 4. 15 Nov. 1659. Pendulum AB semivibrationi impendebat 3/4 unius secundi; filum idem BDC plumbum B et glandem C retinebat, deinde forficubus filum incidebatur, unde necessario eodem temporis articulo globulus C et pendulum moveri incipiebant. plumbum B in F palimsesto impingebatur, ut clarum sonum excitaret. globulus in fundum capsae GH decidebat. simul autem sonabant, cum CE altitudo erat 8 pedum et 9 1/2 unciarum circiter. Sed etsi 3 quatuorve uncijs augeretur vel diminueretur altitudo CE nihilo minus simul sonare videbantur. adeo ut exacta mensura hoc pacto obtineri nequeat. At ex motu conico penduli debebant esse ipsi 8 pedes et 9 1/2 uniciae. unde uno secundo debebunt peragi a plumbo cadente pedes 15. unc. 7 1/2 proxime. Sufficit quod experientia huic mensurae non repugnet, sed quatenus potest eam comprobet. Si plumbum B et globulum C inter digitos simul contineas ijsque apertis simul dimittere coneris, nequaquam hoc assequeris, ideoque tali experimento ne credas. Mihi semper hac ratione minus inveniebatur spatium CE, adeo ut totius interdum pedis differentia esset. At cum filum secatur nullus potest error esse, dummodo forfices ante sectionem immotae teneantur. Penduli AB oscillationes ante exploraveram quanti temporis essent ope horologij nostri. Experimentum crebro repetebam. Ricciolus Almag. 1. 9 secundo scrupulo 15 pedes transire gravia statuit ex suis experimentis. Romanos nimirum antiquos quos a Rhenolandicis non differre Snellius probat."

[65] Member of a maritime nation, Huygens was fully aware of the value and importance of a good timekeeper for navigation, as well as of the financial possibilities of the invention of a marine clock. It is well known that he tried to have his clock patented in England. *Cf.* L. Defossez, *op. cit.*, 115 sq.

[66] The first pendulum clock was constructed by Huygens in 1657; it contains already curved jaws ensuring the isochronism of the (flexible) pendulum. Yet these jaws were not yet mathematically determined, but formed only on the basis of empirical trial and error procedure. It was only in 1659 that Huygens discovered the isochronism of the cycloid and the means of making the bob of the pendulum move along a cycloid.

[67] That of the evolutes of geometrical curves.

[68] *Cf.* L. Defossez, *op. cit.*, 65; on the contemporary attempts of R. Hooke, *cf.* Louise D. Patterson, Pendulums of Wren and Hooke, *Osiris* **10**: 277–322, 1952.

generating circle: these times are to each other as the semi-circumference is to the diameter.[69]

Thus, if we could make a (cycloidal) pendulum, swinging in precisely one second, we would be able to determine the exact time in which a heavy body would descend along its diameter, and therefrom—the spaces traversed being proportional to the squares of time—to calculate the distance of its fall in one second.

The length of such a pendulum—which, besides, needn't be a cycloidal one, because, as Huygens will point it out to Moray,[70] small oscillations of a common (perpendicular) pendulum are performed in practically the same amount of time as those of the cycloidal one—can be easily calculated as soon as we have succeeded in determining the period of a given cycloidal one.

But, as a matter of fact, we do not need to bother ourselves with the actual fabrication of such a pendulum. This because the formula devised by Huygens,

$$g = \frac{4\pi^2 r^2 l}{3600^2} \quad \text{or} \quad T = \pi\sqrt{\frac{l}{g}}$$

has a general value and determines the value of g as a function of the length and the speed of whatever pendulum we may be using. Indeed, it is a rather short and quick pendulum that Huygens has used, a pendulum only 6.18 inches long, and making 4,464 double oscillations per hour. Accordingly, Huygens concluded that the value of g is 31.25 feet, i.e., 981 cm., which is the value that has been accepted ever since.[71]

The moral of this history of the determination of the acceleration constant is thus rather curious. We have seen Galileo, Mersenne, Riccioli endeavoring to construct a time-keeper in order to be able to make an experimental measure of the speed of the fall. We have seen Huygens succeed, where his predecessors had failed, and, by his very success, dispense with making the actual measurement. This, because his timekeeper is, so to say, a measurement in itself; the determination of its exact period is already a much more precise and refined experiment than all those that Mersenne and Riccioli have ever thought of. The meaning and value of the Huygensian circuit—which finally revealed itself as a shortcut—is therefore clear: not only are good experiments based upon theory, but even the means to perform them are nothing else than theory incarnate.

APPENDIX

1. M. Mersenne, *Harmonie Universelle*, Paris, 1636, p. 111 sq.:

Or il faut icy mettre les experiences que nous avons faites très exactement sur ce suiet, afin que l'on puisse suivre ce qu'elles donnent. Ayant donc choisi une hauteur de cinq pieds de Roy, et ayant fait creuser, et polir un plan, nous luy avons donné plusieurs sortes d'inclinations, afin de laisser rouler une boule de plomb, et de bois fort ronde tout au long du plan: ce que nous avons fait de plusieurs endroits différents suivant les différentes inclinations, tandis qu'une autre boule de mesme figure, et pesanteur tombait de cinq pieds de haut dans l'air; et nous avons trouvé que tandis qu'elle tombe perpendiculairement de cinq pieds de haut, elle tombe seulement d'un pied sur le plan incliné de quinze degrez, au lieu qu'elle devroit tomber seize poulces.

Sur le plan incliné de vingt cinq degrez le boulet tombe un pied 5 demi, il devroit tomber deux pieds, un pouce un tiers: sur celuy de trente degrez il tombe deux pieds: il devroit tomber deux pieds et 1/23 car il feroit six pieds dans l'air, tandis qu'il tombe deux pieds 1/2 sur le plan, au lieu qu'il ne devroit tomber que cinq pieds. Sur le plan incliné de 40 degrez, il devroit tomber trois pieds deux pouces 1/2: et l'experience très exacte ne donne que deux pieds, neuf pouces, car lorsqu'on met le boulet à deux pieds dix

[69] *Cf.* Ch. Huygens, *De vi centrifuga* of 1659, *Œuvres* **16**: 276, La Haye, M. Nijhof, 1929.

[70] *Cf.* Christiaan Huygens, Lettre à R. Moray 30 decembre 1661, *Œuvres complètes*, publiées par la Société hollandaise des sciences **3**: 438; La Haye, M. Nijhof, 1890: "Je ne trouve pas qu'il soit nécessaire d'égaler le mouvement du pendule par les portions de le Cycloide pour determiner cette mesure, mais qu'il suffit de le faire mouvoir par des vibrations fort petites, lesquelles gardent assez près l'églite des temps, et chercher aussi quelle longueur il faut pour marquer, par exemple, une demie seconde par le moyen d'une horologe qui soit déjà en train de bien aller, et ajustée avec la Cycloide.

[71] *Cf.* Ch. Huygens, *Œuvres* **17**: 100: "Het getal van de dobbele slaegen die het pendulum in een uyr doen moet, gegeven sijnde, quadreert het selve, en met het quadraat divideert daer mede 12312000000. ende de quotiens sal aenwijsen de lenghde van het pendulum. te weten als men de twee laetste cijffers daer af snijt, soo is het resterende het getal der duijmen die het pendulum moet hebben; de 2 afgesnedene cijffers beteijckenen, het een, de tienden deelen van een duijm die daer noch bij moeten gedaen werden, het ander, de 100ste deelen van een duym, van gelijcken daer bijte doen. Rhynlandse maet.

"bij exempel Een horologe te maecken sijnde diens pendulum 4464 dobbele slagen in een uijr doen sal, het quadraet van 4464 is 19927296, waer mede gedeelt sijnde 12312000000, komt 6/18 ontrent. dat is 6 duijm 1/10 en 8/100 van een duijm. Indien het getal van de heele duijmen meer is als 12 soo moet het door 12 gedeelt werden om te weten hoe veel heele voeten daer in sijn."

pouces loin de l'extrémité du plan le boulet qui se meut perpendiculairement chet le premier; et quand on l'éloigne de deux pieds huit pouces sur le plan, il tombe le dernier: et lorsqu'on l'éloigne de deux pieds neuf pouces, ils tombent instement en mesme temps, sans que l'on puisse distinguer leur bruits.

Sur le plan de quarante cinq degrez il devroit tomber trois pieds et 1/2 un peu davantage, mais il ne tombe que trois pieds, et ne tombera point trois pieds 1/2, si l'autre ne tombe cinq pieds 3/4 par l'air.

Sur le plan de cinquante degrez il devroit faire trois pieds dix pouces, il n'en fait que deux et neuf pouces: ce que nous avons repeté plusieurs fois très exactement, de peur d'avoir failly, à raison qu'il tombe en mesme temps de 3 pieds, c'est à dire de 3 pouces davantage sur le plan incliné de 45 degrez: ce qui semble fort estrange, puisqu'il doit tomber dautant plus viste que le plan est plus incliné: Et néanmoins il ne va pas plus viste sur le plan de 50 degrez que sur celuy de 40: où il faut remarquer que ces deux inclinations sont également éloignées de celle de 45 degrez, laquelle tient le milieu entre les deux extremes, à sçavoir entre l'inclination infinie faite dans la ligne perpendiculaire et celle de l'horizontale: toutefois si l'on considère cet effet prodigieux, l'on peut dire qu'il arrive à cause que le mouvement du boulet estant trop violent dans l'inclination de 50 degrez, ne peut rou'er et couler sur le plan, qui le fait sauter plusieurs fois: dont il s'ensuit autant de repos que de sauts, pendant lesquels le boulet qui chet perpendiculairement, avance toujours son chemin: mais ces sauts n'arrivent pas dans l'inclination de 40, et ne commencent qu'après celle de 45, insques à laquelle la vitesse du boulet s'augmente toujours de telle sorte qu'il peut toujours rouler sans sauter: or tandis qu'il fait trois pieds dix pouces sur le plan incliné de cinquante degrez, il en fait six 1/2 dans l'air au lieu qu'il n'en devroit faire que cinq.

Nous avons aussi experimenté que tandis que la boule fait 3 pieds 10 pouces sur le plan incliné de 50 degrez, elle fait 6 pieds 1/2 par l'air, combien qu'elle ne denst faire que cinq pieds. A l'inclination de 40, elle fait quasi 7 pieds dans l'air, pendant qu'elle fait 3 pieds 2 pouces 1/2 sur le plan; mais l'expérience reiteree à l'inclination de 50, elle fait 3 pieds sur le plan, quoy que la mesme chose arrive à 2 pieds 9 pouces: ce qui monstre la grande difficulté des expe-iences; car il est très difficile d'appercevoir lequel tombe le premier des deux boulets dont l'un tombe perpendiculairement, et l'autre sur le plan incliné. J'ajoûte néanmoins le reste de nos experiences sur les plan inclinez de 60 et de 65 degrez: le boulet éloigné de l'extremité du plan de 2 pieds, 9 pouces, ou de 3 pieds, tombe en mesme temps que celuy qui chet de cinq pieds de haut perpendiculairement, et néanmoins il devroit cheoir 4 pieds 1/3 sur le plan de 60, et 4 pieds 1/2 sur celuy de 65. Sur le plan de 75 il devroit faire 4 pieds 10 pouces, et l'experience ne donne que 3 pieds 1/2.

Peut estre que si les plans ne donnoient point plus d'empeschement aux mobiles que l'air, qu'ils ne tomberoient suivant les proportions que nous avons expliqué: mais les experiences ne nous donnent rien d'asseuré, particulièrement aux inclinations qui passent 45 degrez, parce que le chemin que fait le boulet à cette inclination, est quasi égal à celuy qu'il fait sur les plans de 50, 60, et 65; et sur celuy de 75 il ne fait que demi pied davantage.

F. Mersenne allows himself even to doubt the actual performance by Galileo of some of the experiments mentioned by the great scientist. Thus, referring to the experiments on the inclined plane described by Galileo in his *Dialogo* (not to those described in the *Discorsi*, which I have quoted), he writes (*Harmonie Universelle*, 112, corr. I):

Je doute que le sieur Galilée ayt fait les experiences des cheutes sur le plan puisqu'il n'en parle nullement, et que la proportion qui donne contredit souvent l'experience: et desire que plusieurs esprouvent la mesme chose sur des plans differens avec toutes les précautions dont ils pourront s'aviser, afin qu'ils voyent si leurs experiences respondront aux notres, et si l'on en pourra tirer assez de lumiere pour faire un Theoreme en faveur de la vitesse de ces cheutes obliques, dont les vitresses pourroient estre mesurees par les differens effets du poids, qui frappera dautant plus fort que le plan sera moins incliné sur l'horizon, et qu'il approchera davantage de la ligne perpendiculaire.

2. *Ibid.* 138:

Mais quant à l'expérience de Galilée, on ne peut ni imaginer d'où vient la grande différence qui se trouve icy à Paris et aux environs, touchant le tems des cheutes, qui nous a toujours paru beaucoup moindre que le sien: ce n'est pas que je veuille reprendre un si grand homme de peu de soin en ses expériences, mais on les a faites plusieurs fois de différentes hauteurs, en présence de plusieurs personnes, et elles ont toujours succédé de la mesme sorte. C'est pourquoy si la brasse dont Galilée s'est servy n'a qu'un pied et deux tiers, c'est à dire vingt poulces de pied du Roy dont on use à Paris, il est certain que le boulet descend plus de cent brasses en 5″.

Cecy étant posé, les cent brasses de Galilée font 166 2/3 de nos pieds, mais nos expériences répétées plus de cinquante fois, jointes à la raison doublée, nous contraignent de dire que le boulet fait 300 pieds en 5″, c'est à dire 180 brasses, ou quasi deux fois davantage qu'il ne met: de sorte qu'il doit faire les cent brasses, ou 166 pieds 2/3 en 3″ et 18/25, qui font 3″, 43‴, 20⁗, et non pas 5″; car nous avons prouvé qu'un globe de plomb pesant environ une demie livre, et que celuy de bois pesant environ une once tombent

de 48 pieds en 2″, de 108 en 3″, et de 147 pieds en 3″ et ½. Or les 147 pieds reviennent à 88 et 1/5 brasses; et s'il se trouve du mesconte, il vient plustot de ce que nous donnons trop peu d'espace aux dits temps, qu'au contraire, car ayant laissé cheoir le poids de 110 pieds, il est iustement tombé en 3″, mais nous prenons 108 pour régler la proportion; et les hommes ne peuvent observer la différence du temps auquel il tombe de 110, ou de 108 pieds. Quant à la hauteur de 147 pieds, il s'en fallait un demi-pied, ce qui rend la raison double très-iuste, d'autant que le poids doit faire 3 pieds en une demie seconde, suivant cette vistesse, 12 pieds dans une seconde minute; et conséquemment, 27 pieds en 1″ et ½, 48 pieds en 2″, 75 en 2″ et ½, 108 pieds en 3″ et 147 pieds en 3″ et ½, ce qui revient fort bien à nos experiences, suivant lesquelles il tombera 192 pieds en 4″ et 300 en 5″, pendant lequel Galilée ne met que 166 pieds ou 100 brasses, selon lesquelles il doit faire une brasse en une demie seconde, 4 en 1″, ce qui font près de 6 pieds 2/3, au lieu de 12 que le poids descend en effet.

3. F. M. Mersennus, *Cogitata physico-mathematica*, Phenomena ballistica, Parisii, 1644. Propositio XV. Grauium cadentium velocitatem in ratione duplicata temporum augeri probatur ex pendulis circulariter motis, ipsorúmque pendulorum multifarius usus explicatur, 38–44.

Certum est secundò filum à puncto C ad B cadens temporis insumere tantundem in illo casu, quantum insumit in ascensu à B ad D per circumferentiam BHFD; sit enim filum AB 12 pedum, docet experientia globum B tractum ad C, inde ad B spatio secundi minuti recidere, & alterius secundi spatio à B versus D ascendere. Si verò AB trium pedum fuerit, hoc est praecedentis subquadruplum, spatio dimidij secundi à C descendet ad B, & aequali tempore à B ad D vel S perueniet; ad D si filum & aër nullum afferant impedimentum, cùm impetus ex casu C in B impressus sufficiat ad promouendum globum pendulum ad D punctum.

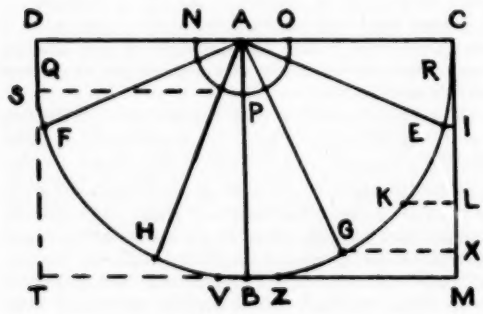


FIG. 1.

Globus igitur spatio secundi percurret dimidiam circumferentiam CBD, & aequali tempore à D per B versus C recurret; donec hinc inde vibratus tandem in puncto B quiescat, siue ab aëris & fili resistentiam vnicuique cursui & recursui aliquid detrahentem, siue ob ipsius impetus naturam, quae sensim minuatur, qua de re postea. Nota verò globum plumbeum vnius vnciae filo tripedali appensum, non priùs quiescere postquam ex puncto C moueri coepit, quàm trecenties sexagies per illam semicircumferentiam ierit; cuius postremae vibrationes à B ad V sunt adeo insensibiles, vt illis nullus ad obseruationes vti debeat, sed alijs maioribus, quales sunt ab F, vel ab H ad B.

Certum est tertiò filum AP fili AB subquadruplum vibrationes suas habere celeriores vibrationibus fili BA; esséque filum AB ad PA in ratione duplicata temporum quibus illorum vibrationes perficiuntur, atque adeo tempora habere se ad filorum longitudines vt radices ad quadrata; quapropter ipsae vibrationes sunt in eadem ac tempora ratione.

Sextò, filum tripedale potest alicui iustò videri longius ad secundum minutum qualibet vibratione notandum, cùm enim in linea perpendulari AB graue cadens citiùs ad punctum B peruenìat, quam vbi ex C vel D per circumferentiae quadrantem movetur, quandoquidem AB linea breuissimè ducit ad centrum grauium, & tamen ex observationibus grauia cadentia tripedale duntaxat interuallum ab A ad B semisecundo, & 12 pedes secundo conficiant, illud filum tripedale minus esse debere videtur: Iamque lib. 2. de causis sonorum, corrollario 3. prop. 27. monueram eo tempore quo pendulum descendit ab A, vel C and B per CGB, posita perpendiculari AB 7 partium, graue per planum horizonti perpendiculare partes vndecim descendere.

Quod quidem difficultatem insignem continet, cùm vtrúmque multis observationibus comprobatum fuerit, nempe grauia perpendiculari motu duodecim solummodo pedes spatio secundi, globum etiam circumferentiae quadrantem, cuius radius tripedalis, à D ad B semisecundo percurrere; fieri tamen nequeunt nisi globus à C ad B per circumferentiae quadrantem descendat eodem tempore quo globus aequalis per AB: qui cùm pedes 5 perpendiculariter descendat eo tempore quo globus à C ad D peruenit, nulla mihi solutio videtur; nisi maius spatium à graui perpendiculariter cadente percurri dicatur quam illud quod hactenus notaueram, quod cùm ab vno quóque possit obseruari, nec vlla velim mentis anticipatione praeiudicare, nolui dissimulare nodum, quem alius, si potis est, soluat. Vt vt sit obseruatio pluries iterata docet tripedale filum nongentesies spatio quadrantis horae vibrari, ac consequenter horae spatio 3600: quapropter si per lineam perpendicularem graue 48 pedes spatio 2 secundorum exactè percurrat, vel fatendum est graue aequali tempore ab eandem altitudine per circuli quadrantem, ac per ipsam perpendicularem cadere, vel aërem magis obsistere grauibus perpendiculariter,

quàm obliquè per circumferentiae quadrantem descendentibus, vel graue plures quàm 12 pedes secundi spatio, aut plusquam 48 duobus secundi descendere, in eo fefellisse observationes, quòd allisio, grauium ad pauimentum aut solum ex audito sono indicata fuerit, qui cùm tempus aliquod in percurrentis 48 pedibus insumat, quo tamen graue non ampliùs descendit, augendum videtur spatium à grauibus perpendiculariter confectum.

Septimò, globus B ex C in B cadens paulò plus temporis quàm ab E, & ab E quàm à G insumit, adeout fila duo equalia, quorum vnum à C, aliud à G suas vibrationes incipiat, quod à G incipit, 36 propemodum uibretur, dum quod à C incipit 35 duntaxat vibratur, hoc est vnam vibratiorem lucretur quod à G cadit, à quo si quamlibet vibrationem inciperet, & aliud suam quamlibet a puncto C, longè cituis illam vibrationem lucraretur. Quantò verò breuiori tempore globus leuoir, verbi gratia suberis, suas vibrationes faciat, quantòque citiùs vibrationum suarum periodum absoluat, lib. 2. de causis sonorum prop. 27 & alijs harmonicorum nostrorum locis reperies.

Duodecimò, pendulorum istorum vibrationes pluribus vsibus adhiberi possunt, vt tractatu de horologio vniversali, & harmonicorum tum Gallicorum 1. 2. de motibus, & alijs pluribus locis, tum Latinorum etiam 1. 2. de causis sonorum à prop. 26. ad 30. dictum est . . . Tantùm addo me postea deprehendisse fili tripedalem longitudinem sufficere, quae sua qualibet vibratione minutum secundum notet, cùm praedictis locis pedibus 3 1/2 vsus fuerim: sed cùm vnusquisque debeat experiri, cum horologio minutorum secundorum exactissimo, filum quo deinceps in suis vtatur obseruationibus, non est quod hac de re pluribus moneavi: adde quòd in mechanicis filum illud siue tripedale, siue pedum 3 1/2 satis exactè secunda repraesentet, vt experientia conuictus fateberis: hinc in soni velocitate reperienda, quae secundo 230 hexapedas tribuit, hoc filo vsus sum, quo medici possint exporare varios singulis diebus aegrotorum, sanorúmque pulsus.

PUBLICATIONS

OF

The American Philosophical Society

The publications of the American Philosophical Society consist of PROCEEDINGS, TRANSACTIONS, MEMOIRS, and YEAR BOOK.

THE PROCEEDINGS contains papers which have been read before the Society in addition to other papers which have been accepted for publication by the Committee on Publications. In accordance with the present policy one volume is issued each year, consisting of six bimonthly numbers, and the price is $5.00 net per volume.

THE TRANSACTIONS, the oldest scholarly journal in America, was started in 1769 and is quarto size. In accordance with the present policy each annual volume is a collection of large monographs each issued as a part. The annual subscription price is $6.00 net per volume; for both the PROCEEDINGS and the TRANSACTIONS $10.00.

Each volume of the MEMOIRS is published as a book. The titles cover the various fields of learning, the recent volumes being largely historical. The price of each volume is determined by its size and character.

The YEAR BOOK is of considerable interest to scholars because of the reports on grants for research and to libraries for this reason and because of the section dealing with the acquisitions of the Library. In addition it contains the Charter and Laws, and lists of present and former members, and reports of committees and meetings. The YEAR BOOK is published about April 1 for the preceding calendar year. The price is $1.50.

A catalogue of the contents of the publications may be had
by addressing the

AMERICAN PHILOSOPHICAL SOCIETY

INDEPENDENCE SQUARE

PHILADELPHIA 6, PA.

Recent Publications of the Society

PROCEEDINGS: Information concerning the articles published in the *Proceedings* may be obtained upon application to the office of the Society.

TRANSACTIONS:

A Revision of Fossil Sequoia and Taxodium in Western North America Based on the Recent Discovery of Metasequoia, by RALPH W. CHANEY, Vol. 40, pt. 3, 93 pp., 12 pls., 1951. $1.50.

The Ancient Khmer Empire, by LAWRENCE PALMER BRIGGS. Vol. 41, pt. 1, 295 pp., 80 figs., 17 maps, 1951. $5.00 paper-bound; $6.00 cloth-bound.

Shaftesbury and the Deist Manifesto, by ALFRED OWEN ALDRIDGE. Vol. 41, pt. 2, 89 pp., 1951. $1.50.

The History of American Ornithology before Audubon, by ELSA G. ALLEN. Vol. 41, pt. 3, 205 pp., 55 figs., 1951. $2.00.

The Comédie Française 1701-1774, by H. CARRINGTON LANCASTER, Vol. 41, pt. 4, 257 pp., 1951. $2.50

Portraits and Miniatures by Charles Willson Peale, by CHARLES COLEMAN SELLERS. Vol. 42, pt. 1, 369 pp., 456 figs., 1952. $5.00.

Floristic Relationships between Eastern Asia and Eastern North America, by HUI-LIN LI. Vol. 42, pt. 2, 59 pp., 56 maps, 1952. $1.00.

Three Revolutionary Architects—Boullée, Ledoux, Lequeu, by EMIL KAUFMANN. Vol. 42, pt. 3, 134 pp., 284 figs., 1952. $2.00.

Menomini Peyotism, by J. S. SLOTKIN. Vol. 42, pt. 4, 136 pp., 14 figs., 1952. $2.00

Historic Philadelphia, from the Founding until the Early Nineteenth Century. Vol. 43, pt. 1, 331 pp., 285 figs., 1 folding map, 1953. $4.00.

MEMOIRS:

Letters and Papers of Benjamin Franklin and Richard Jackson, 1753-1785, edited by Carl Van Doren. Vol. 24, 222 pp., 12 figs., 1947. $3.50.

Autobiography of Benjamin Rush, edited by George W. Corner. Vol. 25, 399 pp., 13 figs., 1948. $6.00.

Benjamin Franklin and Catharine Ray Greene—Their Correspondence, 1755-1790, edited by WILLIAM G. ROELKER. Vol. 26, 147 pp., 10 figs., 1949. $3.00.

The Letters of Benjamin Franklin & Jane Mecom, edited by CARL VAN DOREN. Vol. 27, 380 pp., 8 pls., 1950. $5.00.

William Shippen, Jr., Pioneer in American Medical Education, by BETSY COPPING CORNER. Vol. 28, 161 pp., 9 pls., 1951. $2.75.

West Roman Vulgar Law, by Ernst Levy. Vol. 29, 305 pp., 1951. $5.00.

Letters of Benjamin Rush, edited by L. H. BUTTERFIELD. Vol. 30, Pts. 1 and 2, 1295 pp., 15 figs., 1951. $15.00.

Erwin Frink Smith—A Story of North American Plant Pathology, by ANDREW DENNY RODGERS III. VOL. 31, 675 pp., 4 figs., 1952. $5.00.

The Primitive City of Timbuctoo, by HORACE MINER. Vol. 32, 297 pp., 22 figs., 1953. $5.00.